KBS /PER/1

Digitized by the Internet Archive
in 2019 with funding from
Wellcome Library

https://archive.org/details/s1id13378420

76938

A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. XVII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR

THE AUTHOR,

AND SOLD BY

J. STRATFORD, No. 112, HOLBORN-HILL.

1807.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, W. H. Wollaston, Sec. R. S.; Mr. W. Skrimshire, Jun.; W. W.; Mr. John Tatum, Jun.; Emeritus; R. T.; W. X.

Of Foreign Works, C. A. Prieur; C. L. Cadet; M. Desormes; M. Clement; M. Proust; M. Bouillon Lagrange; Dr. Samuel L. Mitchell; M. Antony Alexis Cadet de Vaux; M. Vauquelin; M. Antony Thillaye-Plat; F. F. Delaroche; M. Lamarck; M. De Lalande; M. Laplace; John Michael Haussman; M. Guyton; Dr. Haldat; M. Henry; M. Darso; M. Gehlen; M. Erman; M. Delaville; M. Dupont de Nemours; M. Planche; M. And. de Gy; M. Chaptal; M. Germon; C. H. Pfaff; M. J. J. Champollion Figeac; M. Roswag; M. Bellemère; Messrs. Von Humboldt and Gay-Lussac; M. Cotte; M. Carnot.

And of British Memoirs abridged or extracted, Benjamin Count of Rumford; Richard Phillips, Esq.; Dr. W. Roxburg; Mr. Charles Wilson; Mr. Robert Salmon; Mr. John Austin; Mr. Jessop; M. G. Field; J. Curwen, Esq. M. P.; Mr. Charles Waistell; Mr. John Trotter; Mr. James Hardie; Rev. James Headrick.

The Engravings consist of 1. Camera Lucida, by W. Hyde Wollaston, M D. Sec. R. S.; 2. Decomposition of Light, by C. A. Prieur; 3, 4. Apparatus of Mr. Thillaye Platel for the Carbonization of Turf; 5. Mr. Tatum's Apparatus for ascertaining the Increase of Temperature by the Galvanic Action; 6. Theory of Looming, or Horizontal Refraction; 7. Mr. Salmon's Specimens of good and bad Pruning of Fir Trees; 8. Mr. Field's Stove for heating or drying; 9. Mr. Waistell's Horse Hoe; 10. Mr. Curwen's Weed Harrow; 11. Apparatus for triturating Quicksilver; 12. Erman's new Classes of Galvanic Conductors; 13. Mr. J. Trotter's Curvilinear Saw; 14. Mr. J. Hardie's Bookbinder's Cutting Press.

TABLE

TABLE OF CONTENTS

TO THIS SEVENTEENTH VOLUME.

JUNE, 1807.

Engravings of the following Objects: 1. Camera Lucida, by W. Hyde Wollaston, M. D. Sec. R. S. 2. Decompostion of Light.

I. Description of the Camera Lucida. By W. H. Wollaston, Sec. R. S. Page 1

II. Description of a new Boiler constructed with a View to the saving of Fuel. By Benjamin Count of Rumford. Read at a Meeting of the first Class of the National Institute the 6th October, 1806. - - 5

III. Notice of an Experiment on the Use of the Heat of Steam, in Place of that of an open Fire, in the making of Soap. By Benjamin Count of Rumford. Read at a Meeting of the First Class of the National Institute, the 20th of October, 1806. - - - - 11

IV. On the Habitudes of Saline Bodies with Regard to Electricity. By Mr. William Skrimshire, Jun. Communicated by Mr. Cuthbertson. - 13

V. On the Decomposition of Light into its most simple Elements: a Fragment of a Work on Colours, by C. A. Prieur, formerly Colonel in the Corps of Engineers, and Lecturer in the National Institution. - 18

VI. Wooden Matches for Artillery, to be used instead of Rope Match, or Port-Fires: read at the National Institute, April 1806. By C. L. Cadet. 31

VII. Letter from a Correspondent on the Means of destroying the Insects which infest the Houses in large Towns. - - - 38

VIII. Theory of the Fabrication of Sulphuric Acid; read in the Class of Physical and Mathematical Sciences of the French National Institute, January the 20th, 1806. By Messrs. Desormes and Clement. - - 41

IX. Facts toward a History of Cobalt and of Nickel. By Mr. Proust; abridged by Mr. Chevreuil. - - - - - 46

X. Facts toward a History of the Gallic Acid. By Bouillon Lagrange. 58

XI. Observations on the Soda, Magnesia, and Lime, contained in Water of the Ocean; showing that they operate advantageously there by neutralizing Acids, and among others the Septic Acid, and that Sea Water may be rendered fit for washing Clothes without the Aid of Soap. By Samuel L. Mitchill, of New York - - - - - 72

XII. An Account of the Improvement of an extensive Tract of Land. By Richard Phillips, Esq. - - - - 74

XIII. Scientific News - - - - - 88

JULY,

JULY, 1807.

Supplementary Number to Vol. XVI.

Engravings of the following Objects: 1. and 2. Apparatus of M. Thillaye Platel for the Carbonization of Turf. 3. Mr. Tatum's Apparatus for ascertaining the Increase of Temperature by the Galvanic Action.

I. Facts toward a History of Prussiates. By Mr. Proust - Page 89

II. A Table of the Growth of Trees in the Botanic Garden at Calcutta. By Dr. Wm. Roxburgh - - - - 110

III. Extract from a Dissertation on Coffee, its History, Properties, and the Mode of obtaining from it the most pleasant, wholesome, and economical Beverage. By Antony Alexis Cadet de Vaux, Member of various Academies: with its Analysis, by Charles Lewis Cadet, Apothecary in ordinary to his Majesty the Emperor, Professor of Chemistry, &c. - 113

IV. Account of the Existence of Platina in the Silver Mines of Guadalcanal, in the Province of Estremadura. By M. Vauquelin - 128

V. Carbonization of Turf, or Process by which all possible advantage may be derived from Products hitherto neglected in that Operation, executed in the Year of the Republic 11. By Antony Thillaye-Platel, House Apothecary at the Hotel-Dieu at Paris. - - - - 131

VI. Method of curing Damp Walls, by the Application of a Composition newly invented by Mr. Charles Wilson, of Worcester Street, near Union Hall, in the Borough - - - - - 141

VII. Experiments on the Effects produced by a High Temperature on the Animal Economy. By F. F. Delaroche, of Geneva - 142

VIII. On the Increase of Temperature produced by the Galvanic Action. By Mr. John Tatum, Jun. - - - 149

Scientific News.—On the Tempest of Feb. 18, which produced many dreadful Accidents in the Channel - - - - 152

JULY, 1807.

Engravings of the following Objects: 1. Theory of Looming or Horizontal Refraction. 2. Mr. Salmon's Specimens of good and bad Pruning of Fir Trees.

I. On Looming, or Horizontal Refraction. By a Correspondent 153

II. Remarks on Pruning Fir Trees; with an Engraving, to explain the Advantages of the Method recommended. By Mr. Robert Salmon, of Woburn, Bedfordshire - - - - - 157

III. Abstract of a Memoir read at the Meeting of the fifth Class of the Institute, September 29th, 1806, by Mr. Laplace, on the apparent Attraction and Repulsion of small Bodies floating on the Surface of Liquids - 164

IV. Abstract of a Memoir on the Adhesion of Bodies to the Surface of Fluids, read at the Sitting of the first Class of the Institute, Nov. the 24th, 1806. By Mr. Laplace - - - - - 169

V. Account of a Loom to be worked by Steam or Water. By Mr. John Austin, of Glasgow - - - - - 175

VI. Observations and Experiments respecting the Art of making Copies of Written Paper by Pressure. By R. T. - - - 178

VII. Of Violet Purple, and the different Tints that may be derived from it. By John Michael Haussman - - - - 182

VIII. On Cast Iron. By Professor Proust - - 185

IX. On Filtering Stones, and the Method of determining the specific Gravity of Substances with large Pores. By Mr. Guyton - - 190

X. Report on a sculptured Head of Flint, with a Covering of Calcedony, made to the Physical and Mathematical Class of the Institute, March 31, 1806. By Mr. Guyton - - - - - 195

XI. Experiments on Double Vision. By Dr. Haldat, Secretary to the Academy of Nancy - - - - - - 201

XII. Experiments on the Effects produced by a high Temperature on the Animal Economy. By F. F. Delaroche, of Geneva - - 215

XIII. Observations on the Two different Methods of preparing Acetic Ether. By Mr. Henry, Professor in the School of Pharmacy at Paris - 219

XIV. Inquiries concerning the Oxidations of Iron. By Mr. Darso 221

XV. Scientific News - - - - 227

AUGUST,

AUGUST, 1807.

Engravings of the following Objects: 1. Mr. Field's Stove for Heating or Drying. 2. Mr. Waistell's Horse Hoe. 3. Mr. Curwen's Weed Harrow.

I. A Memoir on two new Classes of Galvanic Conductors. By Mr. Erman Page 233

II. Facts toward a History of Prussiates. By Mr. Proust - 249

III. An Account of a Stove for Heating Rooms, or Drying different Articles. By Mr. G. Field, of Newman Street - - - 263

IV. Inquiries concerning the Oxidations of Iron. By Mr. Darso - 267

V. Account of Mr. Curwen's Drill Horse Hoe, or Weed Harrow 281

VI. Account of a Drill Horse Hoe for Turnips, communicated by Mr. Charles Waistell - - - - - - 284

VII. On Capillary Action. By Mr. Laplace - - 286

VIII. Letter from Mr. Delaville, M. D. of Cherbourg, to Mr. Vauquelin, Member of the Institute, on the Oxidation of Metals, and particularly on that of Lead - - - - - - 297

IX. An Essay on Instinct, read to the French National Institute. By Mr. Dupont de Nemours - - - - - 229

X. Observations on the Sulphurous Acid. By Mr. Planche. Read to the Society of Pharmacy, November the 15th, 1806 - - 303

XI. Scientific News: French National Institute - - 306

SUPPLEMENT

SUPPLEMENT TO VOL. XVII.

Engravings of the following Objects: 1. Apparatus for triturating Quicksilver: 2. Erman's New Classes of Galvanic Conductors: 3. Mr. J. Trotter's Curvilinear Saw: 4. Mr. J. Hardie's Bookbinder's Cutting Press.

I. Description of a Machine for triturating and combining Quicksilver with other Substances. By a Correspondent - - - 313

II. A Memoir on Two new Classes of Galvanic Conductors. By Mr. Erman 316

III. Inquiries concerning the Oxidations of Iron. By Mr. Darso 328

IV. Description of a Curvilinear Saw, invented by John Trotter, Esq., of Soho Square - - - - - - 334

V. Account of a Bookbinder's Cutting Press, for which Fifteen Guineas were voted to Mr. James Hardie, of Glasgow, by the Society of Arts 336

VI. On Blende, and some other Articles. By Professor Proust - 337

VII. Remarks on the Structure of Mount Jura, from a considerable Number of Heights taken by the Barometer, and extended through France to the Sea. By Mr. And. de Gy, Member of the Academy of Cassel, &c. 341

VIII. Some Mineralogical and Geological Observations, made in the Isle of Arran. By the Rev. James Headrick - - - - 344

IX. Questions respecting the Vines and Wines of Champagne, by Mr. Chaptal, with Answers to them by Mr. Gernon, of Epernay - 353

X. On the most sensible Reagents for Muriatic, Carbonic, and Sulphuric Acids, and for Ammonia. By C. H. Pfaff, Professor of Chemistry at Kiel. 360

XI. Some farther Remarks on the pretended Formation of Muriatic Acid in Water by the Influence of the Galvanic Pile. By the same - 362

XII. Description of the Mode of making Threshing-Floors in the Commune of Valbonnais, in the Department of the Isère. By Mr. J. J. Champollion Figeac, Secretary to the Society of Sciences and Arts at Grenoble, &c. 363

XIII. Scientific News—French National Institute - - 365

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

JUNE, 1807.

ARTICLE I.

Description of the Camera Lucida. *By* W. H. Wollaston, *Sec. R. S.*

HAVING a short time since amused myself with attempts to sketch various interesting views, without an adequate knowledge of the art of drawing, my mind was naturally employed in facilitating the means of transferring to paper the apparent relative positions of the objects before me; and I am in hopes that the instrument, which I contrived for this purpose, may be acceptable even to those who have attained to greater proficiency in the art, on account of the many advantages it possesses over the *Camera Obscura*.

New instrument for delineation.

The principles on which it is constructed will probably be most distinctly explained by tracing the successive steps, by which I proceeded in its formation.

Principles.

While I look directly down at a sheet of paper on my table, if I hold between my eye and the paper a piece of plain glass, inclined from me downwards at an angle of 45°, I see by reflection the view that is before me, in the same direction that I see my paper through the glass. I might then take a sketch of it; but the position of the objects would be reversed.

Objects seen as if transferred to paper, by reflection from transparent glass.

To obtain a direct view, it is necessary to have two reflections.

By a single reflection are inverted;

two reflections give the natural position.

flections. The transparent glass must for this purpose be inclined to the perpendicular line of sight only the half of 45°, that it may reflect the view a second time from a piece of looking glass placed beneath it, and inclined upwards at an equal angle. The objects now appear as if seen through the paper in the same place as before; but they are direct instead of being inverted, and they may be discerned in this manner sufficiently well for determining the principal positions.

The objects and the paper cannot be distinctly seen at once.

The pencil, however, and any object, which it is to trace, cannot both be seen distinctly in the same state of the eye, on account of the difference of their distances, and the efforts of successive adaption of the eye to one or to the other, would become painful if frequently repeated. In order to remedy this inconvenience, the paper and pencil may be viewed through a convex lens of such a focus, as to require no more effort than is necessary for seeing the distant objects distinctly. These will then appear to correspond with the paper in *distance* as well as *direction*, and may be drawn with facility, and with any desired degree of precision.

Arrangement of the glasses.

This arrangement of glasses will probably be best understood from inspection of Fig. I. *a b* in the transparent glass; *b c* the lower reflector; *b d* a convex lens (of twelve inches inches focus) *e* the position of the eye; and *f g h c* the course of the rays. See Pl. I.

Another construction.

In some cases a different construction will be preferable. Those eyes, which without assistance are adapted to seeing near objects alone, will not admit the use of a convex glass; but will on the contrary require one that is concave to be placed in front, to render the distant objects distinct. The frame for a glass of this construction is represented at *i k*, (fig 3.) turning upon the same hinge at *h* with a convex glass in the frame 1 m, and moving in such a manner, that either of the glasses may be turned alone into its place, as may be necessary to suit an eye that is long or short sighted. Those persons, however, whose sight is nearly perfect, may at pleasure use either of the glasses.

Difference in the latter in-

The instrument represented in that figure differs moreover in other respects from the foregoing, which I have chosen

to describe first, because the action of the reflectors there employed would be more generally understood. But those who are conversant with the science of optics will perceive the advantage that may be derived in this instance from prismatic reflection; for when a ray of light has entered a solid piece of glass, and falls from within upon any surface, at an inclination of only twenty-two or twenty-three degrees, as above supposed, the refractive power of the glass is such as to suffer none of that light to pass out, and the surface becomes in this case the most brilliant reflector that can be employed.

strument. Prismatic reflection.

Fig. 2. represents the section of a solid prismatic piece of glass, within which both the reflections requisite are effected at the surfaces *a b*, *b c*, in such a manner that the ray *f g*, after being reflected first at *g*, and again at *h*, arrives at the eye in a direction *h e* at right angles to *f g*.

Figure of the prism.

There is another circumstance in this construction necessary to be attended to, and which remains to be explained. Where the reflection was produced by a piece of plain glass, it is obvious that any objects behind the glass (if sufficiently illuminated) might be seen through the glass as well as the reflected image. But when the prismatic reflector is employed, since no light can be transmitted directly through it, the eye must be so placed that only a part of its pupil may be intercepted by the edge of the prism, as at *e* Fig. 2. The distant objects will then be seen by this portion of the eye, while the paper and pencil are seen past the edge of the prism by the remainder of the pupil.

Remedy for the want of transmission of direct light through the prism.

In order to avoid inconvenience that might arise from unintentional motion of the eye, the relative quantities of light to be received from the object, and from the paper are regulated by a small hole in a piece of brass, which by moving on a center at *c*, fig. 3. is capable of adjustment to every inequality of light that is likely to occur.

Since the size of the whole instrument, from being so near the eye, does not require to be large, I have on many accounts preferred the smallest size that could be executed with correctness, and have had it constructed on such a scale, that the lenses are only $\frac{3}{4}$ of an inch in diameter.

The instrument is of small dimensions.

Though the original design, and principal use of this instrument

It may be used in copying drawings.

strument is to facilitate the delineation of objects in true perspective, yet this is by no means the sole purpose to which it is adapted; for the same arrangement of reflectors may be employed with equal advantage for copying what has been already drawn, and may thus assist a learner in acquiring at least a correct outline of any subject.

nstructions.

For this purpose the drawing to be copied should be placed as nearly as may be at the same distance before the instrument that the paper is beneath the eye-hole, for in that case the size will be the same, and no lens will be necessary either to the object, or to the pencil.

It answers every purpose of the pentagraph.

By a proper use of the same instrument, every purpose of the pentagraph may also be answered, as a painting may be reduced in any proportion required, by placing it at a distance in due proportion greater than that of the paper from the instrument. In this case a lens becomes requisite for enabling the eye to see at two unequal distances with equal distinctness, and in order that one lens may suit for all these purposes, there is an advantage in carrying the height of the stand according to the proportion in which the reduction is to be effected.

Method of using the instrument for this purpose.

The principles on which the height of the stem is adjusted will be readily understood by those who are accustomed to optical considerations. For as in taking a perspective view the rays from the paper are rendered *parallel*, by placing a lens at the distance of its *principal* focus from the paper, because the rays received from the distant objects are *parallel;* so also when the object seen by reflection is at so short a distance that the rays received from it are in a certain degree *divergent*, the rays from the paper should be made to have the same degree of divergency in order that the paper may be seen distinctly by the same eye; and for this purpose the lens must be placed at a distance less than its principal focus. The stem of the instrument is accordingly marked at certain distances to which the conjugate foci are in the several proportions of 2, 3, 4, &c. to 1, so that distinct vision may be obtained in all cases, by placing the painting proportionally more distant.

Magnified designs.

By transposing the convex lens to the front of the instrument and reversing the proportional distances, the artist might also enlarge his smaller sketches with every desirable

degree

degree of correctness, and the naturalist might delineate minute objects in any degree magnified.

Since the primary intention of this instrument is already, in some measure, answered by the *Camera Obscura*, a comparison will naturally be made between them.

The objections to the Camera Obscura are

1st. That it is too large to be carried about with convenience.

Comparison of the camera lucida with the camera obscura.

The Camera lucida is as small and portable as can be wished.

2dly. In the former, all objects that are not situated near the centre of view are more or less distorted.

In this, there is no distortion; so that every line, even the most remote from the centre of view, is as strait as those through the centre.

3dly. In that, the field of view does not extend beyond 30° or at most 35° with distinctness.

But in the *Camera Lucida* as much as 70° or 80° might be included in one view.

As it has been thought advisable to secure an exclusive sale of this instrument by patent, those who are desirous of purchasing it are informed that Mr. Newman (No. 24 Soho Square) has at present the disposal of it.

II.

Description of a new Boiler constructed with a View to the saving of Fuel. By Benjamin Count of Rumford. *Read at a Meeting of the first Class of the National Institute the 6th October*, 1806*.

Boiler for generating steam; having its bottom terminating in tubes.

IT is well known that much is gained in the saving of fuel, when an extensive surface is given to that part of the boiler against which the flame strikes, but this advantage is often counterbalanced by great inconveniences. For a boiler of the form usually employed, having the bottom very much extended in proportion to its capacity, must necessarily present a great surface to the atmosphere, and the loss of heat, occasioned by the cold air coming in contact with

* Translated by W. Caddel Esq. and revised by the count, from whom it was received.

this

Boiler for generating steam; having its bottom terminating in tubes.

this surface, may be more than sufficient to compensate the advantage derived from the extended surface of the bottom. And where the boiler is employed for producing steam, as it is indispensably necessary that it should be of a thickness sufficient to resist the expansive force of the steam, it is evident, that if the diameter be augmented (with a view to increase the surface of the bottom) a considerable expence is incurred on account of the additional strength that must be given to the sides.

Having been engaged in the year 1796, in a set of experiments, in which I employed the steam of boiling water as a vehicle of heat; I had a boiler made for this purpose, on a new construction, which answered well, and even beyond my expectations; and, as this boiler might be used with advantage in many cases, even where it is only required to heat liquids in an open boiler, this, and another motive, which it would be useless to mention in this place, have lately induced me to construct one here (at Paris) and to present it to the Institute.

The object chiefly had in view in the construction of this boiler, was to give it such a form, that the surface exposed to the fire should be great in comparison with its diameter and capacity; and this without having a great surface exposed to the cold air of the atmosphere.

The body of the boiler is in the shape of a drum. It is a vertical cylinder of copper twelve inches in diameter, and twelve inches high, closed at top and at bottom by circular plates.

In the centre of the upper plate there is a cylindrical neck six inches in diameter, and three inches high, shut at top by a plate of copper three inches in diameter and three lines in thickness, fastened down by screws.

This last plate is pierced by three holes, each about five lines in diameter. The first, which is in the center of the plate, receives a vertical tube, which conveys water to the boiler from a reservoir which is placed above. This tube, which descends in the inside of the boiler, to within an inch above the circular plate which forms its bottom, has a cock near its lower end. This cock is alternately opened

and

and shut, by means of a floater which swims on the surface of the water contained in the body of the boiler.

Boiler for generating steam; having its bottom terminating in tubes.

The second of the holes in the plate that closes the neck of the boiler, receives the lower end of another vertical tube, which serves to convey the steam from the boiler to the place where it is to be used.

The third hole is occupied by a safety valve.

This description shews that there is nothing new in the construction or arrangement of the upper part of this boiler. In its lower part there is a contrivance for increasing its surface, which has been found very useful.

The flat circular bottom of the body of the boiler, which as I said before is twelve inches in diameter, being pierced by seven holes, each three inches in diameter, seven cylindrical tubes of thin sheet copper, three inches in diameter, and nine inches long, closed below by circular plates, are fixed in these holes, and firmly rivetted, and then soldered to the flat bottom of the boiler.

On opening the communication between the boiler and its reservoir, the water first fills the seven tubes, and then rises to the cylindrical body of the boiler; but it can never rise above six inches in the body of the boiler, for when it has got to that height, the floater is lifted to the height necessary for shutting the cock that admits the water.

When the height of the water in the boiler is diminished a few lines by the evaporation, the floater descends a little, the cock is again opened, and the water flows in again from the reservoir.

As the seven tubes that descend from the flat bottom of the body of this boiler into the fire place, are surrounded on all sides by the flame, the liquid contained in the boiler is heated, and made to boil in a short time, and with the consumption of a relatively small quantity of fuel; and when the vertical sides of the body of the boiler, and its upper part are suitably enveloped, in order to prevent the loss of heat by these surfaces, this apparatus may be employed with much advantage in all cases where it is required to boil water for procuring steam.

And as in the case where the boiler is constructed on a great scale, the seven tubes that descend from the bottom

of

Boiler for generating steam; having its bottom terminating in tubes.

of the boiler into the fire may be made of cast iron, whilst the body of the boiler is composed of sheet iron, or sheet copper; it is certain that a boiler of this kind, sufficiently large for a steam engine, a dying house, or a spirit distillery, would cost much less than a boiler of the usual form, of equal surface and power.

But in all cases where it is required to produce a great quantity of steam, it will be always preferable to employ several boilers of a midling size, placed beside each other, and heated each by a separate fire, instead of using one large boiler heated by one fire.

I have shewn, in my sixth essay, on the management of fire, and the œconomy of fuel, that beyond a certain limit, there is no advantage derived from augmenting the capacity of a boiler.

It will be perceived, that the boiler which I have the honour of presenting to this Society, is of a form fit for being placed in a portative furnace, and it was actually intended for that purpose.

Its furnace, which is made of bricks, with a circular iron grate of six inches in diameter, is built in the inside of a cylinder of sheet iron, seventeen inches in diameter, and three feet high, and can be easily transported from place to place, by two men.

This cylinder of sheet iron, which is divided into two parts, in order to facilitate the construction of the masonry, weighs only forty-six pounds. The masonry weighs about a hundred and fifty pounds, and the boiler twenty-two pounds.

In order to form an estimate of the advantage which the particular form of this boiler gives it in accelerating its heating, we may compare the extent of surface that it presents to the action of the fire, with that of the flat bottom of a common boiler.

The diameter of the bottom of a cylindrical boiler being twelve inches, the surface is 113.88 square inches; but the surface of the sides of the seven tubes that descend from the flat bottom of our boiler (which is likewise twelve inches in diameter) is 593.76 square inches. Therefore, the new boiler has a surface exposed to the direct action of

the

the fire, more than five times greater than that of a boiler of equal diameter, and of the ordinary form: how much this difference must affect the celerity of heating is easy to conceive.

Boiler for generating steam; having its bottom terminating in tubes.

In the manner in which boilers are usually set, their vertical sides are but little struck by the flame, and on that account, I have not taken the effect of the sides into consideration in my estimate; but even taking them into account, the new boiler will always have a surface exposed to the fire, at least twice as great as that of a common cylindrical boiler of the same diameter, as can easily be shewn.

The new boiler being twelve inches in diameter, and twelve inches high, and each of its seven tubes being three inches in diameter, and nine inches high, its surface is 1160.44 square inches, without reckoning the circular plate that closes its top, nor its neck.

The surface of the bottom and sides of a cylindrical boiler of twelve inches in diameter, and twelve inches high, will be 566.68 square inches.

As the quantity of heat that enters a boiler in a given time, is in proportion to the extent of surface that the boiler presents to the fire, it is evident, that other circumstances being the same, a boiler with tubes descending from its bottom, will be heated at least twice as soon as a cylindrical boiler of the same diameter, with a flat bottom.

In order that a cylindrical boiler with flat bottom, surrounded by flame on all sides, might have the same extent of surface exposed to the fire as a boiler with tubes, it would be necessary to give it a diameter greater than that of the boiler with tubes in the proportion of the square root of 1160.44, to the square root of 566.68, that is, of 17.171 to 12.

Therefore, in order that a cylindrical boiler with a flat bottom, might have the same extent of surface exposed to the fire as our boiler with tubes, of twelve inches in diameter, it would be necessary to give it a diameter of 17.171 inches.

But if the diameter of a boiler intended for producing steam be increased, it is necessary, at the same time, to increase its thickness, in order to increase its strength.

The

Boiler for generating steam; having its bottom terminating in tubes.

The necessary increase of thickness, and the expence that it will occasion, can be easily calculated.

The effort that an elastic fluid exerts against the sides of the containing vessel, is in proportion to the surface of a longitudinal and central section of the vessel, and consequently in proportion to the square of its diameter, the form remaining the same. Hence we may conclude, that a steam boiler of a cylindrical form with a flat bottom, which has the same extent of surface exposed to the fire as a boiler of twelve inches in diameter with tubes, should be at least twice as thick as this last, in order to have an equal degree of strength for resisting the expansive power of the steam.

The boiler which I have the honour of presenting to the Society, is particularly intended to serve as a steam boiler, but it may undoubtedly be applied to other purposes. Having shewn it to M. Auzilly, son of a considerable soap manufacturer of Marseilles, he thought that it might be employed with advantage in the making of soap; and from what he told me of the process, and of the boilers employed in that art, I am persuaded that the experiment would succeed perfectly.

But after all, it remains to be determined, whether it would not be still more advantageous to employ steam as a vehicle of heat in the making of soap, instead of lighting the fire under the bottom of the vessel in which the soap is made.

The result of an experiment which we are to make, M. Auzilly and myself, will probably throw some light upon this question.

III.

Notice of an Experiment on the Use of the Heat of Steam, in Place of that of an open Fire, in the making of Soap, By Benjamin Count of Rumford. *Read at a Meeting of the First Class of the National Institute, the 20th of October*, 1806.

Experiment shewing the advantage of

I HAD the honour of announcing to this assembly, at the last meeting but one, that M. Auzilly and myself, were to make

make an experiment on the use of steam in the making of soap. This experiment we have made, and with perfect success. heating soap lees by steam.

I have the honour to lay before the Society, a piece of soap of about ten cubic inches, made in my laboratory by this new process, which required only six hours of boiling, whereas sixty hours and more are necessary in the ordinary method of making soap.

From all the appearances that we observed in the course of this experiment, and from its results, we think ourselves authorised to conclude, that this new method of making soap cannot fail to be advantageous in every respect, and that it will soon be generally adopted.

We propose to repeat the experiment on a larger scale, as soon as we shall be able to procure the necessary utensils, and we beg the Society to appoint commissioners to be present during its execution.

As I intend to communicate to the Institute, upon a future occasion, all the details of our experiment, with an account of the apparatus we employed in it, I shall for the present make only one observation on the probable cause of the acceleration of the formation of soap, which we observed. I believe that this acceleration is due, in great measure, if not entirely, to a motion of a peculiar kind in the mixture of oil and lye, occasioned by the sudden condensation of the steam introduced into the liquor. It is a sharp stroke, like that of a hammer, which made the whole apparatus tremble.

These strokes, which succeeded rapidly in certain circumstances, and which were violent enough to be heard at a considerable distance, must necessarily have forced the particles of oil and alkali to approach each other, and consequently to unite.

As the violence of these strokes diminished greatly as soon as the liquid had acquired nearly the temperature of the steam, I propose to supply this defect by a particular arrangement of the apparatus in the experiment we are going to make. I shall divide the vessel into two parts, by a horizontal diaphragm of thin sheet copper, and causing a slow current of cold water to pass through the lower division

division or compartment of the vessel, I shall introduce steam into it, through a particular tube destined for that purpose, as soon as the mixture of oil and alkali which occupies the upper division of the vessel is become too hot for condensing the steam.

The steam which enters the water (always kept cold) that fills the lower compartment of the vessel, will be condensed suddenly, and the sharp strokes which result will be communicated through the thin diaphragm to the hot liquid contained in the upper division of the vessel, and will, I expect, accelerate the union of the oil with the alkali. I shall then shut almost entirely the cock which admits steam into the upper division of the vessel, in order to prevent an useless consumption of steam and heat.

I shall not fail to give an account of the results of this new experiment to this assembly; and I shall rejoice if by any researches I shall be so happy as to contribute to the improvement of an art which is undoubtedly of great importance to society.

IV.

On the Habitudes of Saline Bodies with Regard to Electricity. By Mr. William Skrimshire, *Jun. Communicated by Mr.* Cuthbertson.

Dear Sir,

HAVING made some further progress in my electrical experiments, I take the liberty of sending you the results, in order, if you think proper, for insertion in Mr. Nicholson's valuable Journal.

Wm. SKRIMSHIRE, Jun.

To Mr. Cuthbertson.

Saline Substances.

Causes of error in taking the spark from saline crystals.

WHEN the spark is taken from saline substances placed upon the conductor, there is some difficulty in ascertaining, whether the spark proceeds from the salt itself, or from the conductor, through the substance, or along the surface of the salt. A large crystal will sometimes appear to give a very

very brilliant spark ramified upon its surface; but if these ramifications be minutely examined, they will be found to proceed from the conductor, and running up the sides of the crystal converge to a point under the knob of the discharger.

Whenever it is doubtful whether a crystal affords a spark or not, I place a second crystal upon the first, and then apply the discharger to the uppermost, when in general, merely a hissing stream of electric light, or at most, a small hissing spark is perceived. Again, if the crystal be thin, it will appear to give as good a spark as any metal, and in truth the spark really proceeds from the conductor, and passing through the salt renders it transparent, or rather semi-transparent. A thin cake of agglutinated crystals allows the spark to pass through its interstices with the same appearance. It is necessary here to remark, with respect to passing the shock through saline substances, that if the salt be crystallized in large lumps like alum or borax, it is shivered in pieces by the shock; and the same happens when the lump consists of a congeries of regular crystals; but if the shock (meaning the shock which I constantly employ in these experiments, and which is never more than that from a quart phial) be passed through a single crystal, no such effect occurs, for the crystal remains perfectly whole, and is generally rendered luminous throughout, should the salt prove phosphoric by the electric light. Remedies, &c.

Alkalies and their Compounds.

Sub-carbonate of potash. 1st. Pearl ash gives a dense purple stream of electric light, instead of a spark, and is extremely phosphoric by the shock, its light continuing some minutes. 2nd. Salt of tartar is very luminous, and its particles are easily scattered by the shock, when the points of the dischargers are in contact with it. Alkali and alkaline salts.

Super-carbonate of potash in small crystals is luminous, and scattered about, if the rods touch it; in larger crystals it is also luminous, but they are not fractured by passing the shock through them.*

* I am inclined to suspect that the shock does not pass through, but over the surface of a single crystal, if it be a small crystal.

Sulphate

Alkali and alkaline salts.

Sulphate of potash affords a small spark, and is luminous by the shock.

Nitrate of potash, commonly called nitre or salt-petre, affords a spark which is beautifully flame-coloured on its surface; it is also luminous, but its light is of short duration.

Muriate of potash is much more phosphorescent than nitre, and its light is of longer continuance.

Hyper-oxymuriate of potash is lumious, but does not explode when the shock is passed through it.

Acidulous oxalate of potash is luminous by the electric shock.

Acidulous tartrite of potash. Red and white argol, and purified crystallized cream of tartar afford similar results, except that cream of tartar is rather more luminous than the others. They do not give a spark; but a cake of conglomerated crystals allows a spark to pass through it, rendering it almost transparent.

Neutral tartrite of potash, or soluble tartar of the apothecaries, is rendered luminous by the explosion.

Tartrite of potash and soda, or Rochelle salt of the apothecaries, affords a beautiful spark, flame coloured, and ramified upon the surface, when a single crystal is made use of; but when one crystal is placed upon another, and the discharger applied to the uppermost, only a purple hissing stream or a very slight spark can be taken from it. It is luminous when the shock is passed *above* its surface.

Acetite of potash gives a purple spark, flame coloured and ramified on its surface, even when one crystal is placed upon another. It is rendered luminous merely by taking the spark from the conductor near it; and by exposure to the shock it is beautifully phosphorescent, shining with a green light, and is even superior in brilliancy to the sulphuret of lime, though its light is of shorter duration than in that preparation. This salt is extremely deliquescent, in which state, and even when quite dissolved, it is still rendered luminous by exposure to the light of the explosion.

Soda. Sub-carbonate of soda affords a hissing purple spark, flame coloured on its surface. It is phosphorescent by the shock.

Sulphate

Sulphate of soda gives a purple spark, and is luminous by the shock. Alkali and alkaline salts.

Nitrate of soda is not at all luminous, even when the points of the discharging rods are in contact with it, during the explosion.

Muriate of soda. Several native specimens of different colours were tried, and gave only a small stream of electric light, almost without any sound. They were all luminous by the shock, as was also common culinary salt.

Phosphate of soda affords only a purple hissing stream, but is luminous by the shock, though in a less degree than the sub-carbonate.

Sub-borate of soda. 1st. Tincal gives a purple hissing spark, apparently proceeding from a red point upon its surface. It is but slightly luminous by the shock. 2nd. East Indian borax gives no spark, but the electric fluid glides silently over every part of its surface from the conductor to the knob of the discharger. Its phosphorescency by the shock is superior to tincal, but not equal to the refined borax of the shops. 3rd. Refined borax affords a hissing purple spark, sometimes flame coloured upon its surface; but when a *large* piece is placed on the conductor it allows only a purple stream to be drawn from it. It is luminous when the explosion is made *above* it, but when the shock is passed *through it*, the phosphoric appearance is very brilliant and has a greenish tint, the light is of short continuance, and the salt is shattered to pieces.

Ammonia. Aqua ammoniæ of the shops is not luminous.

Carbonate of ammonia affords a dense purple spark, radiated on its surface. It is very luminous with a white light, when the shock is taken *above* it; but it possesses a delicate blue or rather purple tint, when the rods rest *upon* its surface, and it is shattered into luminous pieces when the shock is passed *through* it.

Sulphate of ammonia gives a purple hissing stream and is luminous by the shock.

Nitrate of ammonia is not at all luminous when properly prepared and crystallized; but it is slightly phosphoric when prepared with the common aqua fortis of the shops, and not carefully crystallized.

Muriate

Muriate of ammonia affords a purple spark, and is luminous by the explosion.

Succinate of ammonia is luminous when the shock is passed above it, and is readily dispersed in luminous particles when the rods are placed in contact with it.

Acids.

Acids. Sulphuric, nitric, muriatic, phosphoric and acetous acids are not luminous.

Nitric acid affords only a hissing stream instead of a spark. It is extremely phosphorescent with the explosion made *above* it, shining with a greenish light; and when the shock is passed *through* a lump of crystals it is fractured into numerous phosphoric pieces. It is rendered luminous merely by taking sparks from the conductor in its vicinity.

Boracic acid is next in phosphorescency to the citric.

Benzoic acid is almost equally luminous with the boracic.

Tartaric acid affords a hissing stream instead of a spark. It is luminous by the shock, but not quite so phosphoric as the benzoic acid.

Oxalic acid is also luminous by the explosion, but less so than any of the crystallizable acids here mentioned.

Arsenious acid gives no spark, but allows the fluid to pass freely over its surface from the conductor to a considerable distance, giving the sensation of a shock when held in the hand; but when placed upon the conductor and the knob of the discharger rests upon its surface, the spark proceeds from the conductor to the discharger, through the substance of the acid, rendering it semi-transparent. It is very phosphorescent with a white light, when the explosion is made *above* its surface; but when the points of the rods rest *upon* it, the light is yellow tinged with green.

Metallic Salts.

Metallic salts. Nitrate of silver, commonly called lunar caustic, does not give a spark, neither is it luminous by the electric light.

Sulphate of mercury, called turpeth mineral, and white crystallized nitrate of mercury are not luminous.

Howard's fulminating mercury is not phosphorescent by passing the shock above it; but when a grain or two of this salt

salt are placed in the track of the discharge between the points of the rods it explodes with a slight shock. The explosion is accompanied with a dark red or crimson coloured flame and slight detonation. It is a very pleasing experiment, and when exploded upon a plate of glass the mercury is revived, and silvers the plate like a mirror, but it is easily effaced. When exploded upon card, a coloured stain is produced, which is indelible. Metallic salts.

Muriate of mercury, called calomel, is not luminous.

Oxymuriate of mercury, or corrosive sublimate of mercury, affords only a purple stream on its fracture, but the smooth convex surface which has formerly been attached to the vessel it was sublimed in gives a fine purple spark, of a beautiful bright green colour on its surface. It is very phosphorescent by passing the shock above it. Great caution is required in making experiments with this substance, as electricity detaches from it, and throws into the atmosphere, innumerable minute and invisible particles of this caustic poisonous salt, which produce inflammation of the membrane lining the nose, and a very disagreeable sensation in the mouth and fauces, attended with a slight salivation.—Similar effects occur during the explosion of the fulminating mercury, if frequently repeated.

Sulphate, nitrate, and phosphate of copper are not luminous, neither do they give any spark.

Acetite of copper, verdigris of the shops, gives a spark ramified upon its surface, but is not luminous by the electric explosion.

Carbonate or rust of iron gives a spark, but is not luminous.

Sulphate, gallate, and prussiate of iron are not luminous.

Carbonate and acetite of lead give no spark, nor are they luminous.

Muriate of lead is slightly luminous by the explosion.

Sulphate of zinc affords a small purple stream instead of a spark. It is not luminous when the explosion is made *above* its surface, but merely in the track of the fluid when the rods rest upon it, at some distance from each other.

Carbonate of zinc or calamine. Several native specimens

of this substance covering crystals of a calcareous spar, as well as the levigated calamine of the shops, were phosphorescent.

Muriate of antimony is not luminous.

Phosphate of lime and antimony, or James's powder of the apothecaries is very phosphorescent, its light continuing for some minutes.

Tartrite of potash and antimony, or emetic tartar of the apothecaries, is luminous, but not comparable with James's powder.

Miscellaneous Sabstances.

Compounds containing salts.

Soaps.—Common hard white and brown soaps afford very good sparks, which are sometimes flame-coloured on the surface; but they are not luminous even in the track of the fluid upon their surfaces. Neither is the common soft soap at all luminous.

Sulphuret of potash gives a purple hissing stream, and is not luminous, in which it totally differs from the sulphuret of lime.

Common fulminating powder is not luminous, neither does it explode with the shock which I employ in these experiments.

Gun powder is not rendered luminous by the electric light, nor does explode with a small shock.

In my first letter correct—Vol. XV. p. 281, l. 7 from bottom, after *surface*, insert *and lastly along its surface.*—P. 282, l. 3. from bottom, for *sulphate* r. *sulphuret.*

V.

On the Decomposition of Light into its most simple Elements; a Fragment of a Work on Colours: by C. A. PRIEUR, *formerly Colonel in the Corps of Engineers, and Lecturer in the National Institution.**

White light decomposed into different colours,

WHITE light is decomposed by refraction into an infinite number of parts or rays. They have a different co-

* Abridged from the Annales de Chimie for Sept. 1806, p. 227.

lour

lour at every point in the length of the spectrum, and this colour cannot be varied by a new refraction, if the simplification of the spectrum be at the degree to which Newton carried it. Though in the spectrum thus simplified the lines of demarcation between the colours are by no means very perceptible; it is impossible to ascribe the gradations of their tints to one and the same law. Numerous observations establish the existence of several distinct species of colours; and their division into seven classes, as given by Newton, agrees with a great number of phænomena. Yet some substances, by their peculiar refractive power, derange the spaces of the colours in the spectrum; so that, for example, the green rays are in some instances brought nearer the red, in others nearer the violet. This proves, that the dispersion of the rays does not depend absolutely on their own nature. These are the principal observations it appeared to me necessary to make, in order to shew the present state of our knowledge; and I shall now proceed to examine the action of coloured bodies upon light.

which are really distinct;

but not always refracted in an equal degree, though always in the same order.

I have formerly shewn, that all kinds of transparent bodies, of different colours, which I have observed, transmit ultimately only on the red, or green, or violet rays. The progressive absorption never finishes by any other colours, and I long sought in vain for a substance, in which the final absorption should be of the yellow or blue rays.

The yellow and blue rays disappear first.

Such a result could not fail to excite my attention. I remarked, that, under certain circumstances, the colours exhibited by refraction were almost wholly these three, red, green, and violet: that sometimes yellow appeared to arise from a mixture of red and green, and blue from a mixture of green and violet; which my dial,* as well as the placing of certain coloured glasses on each other, indicated as possible. I perceived too, that the tints of the seven orders of colours might be imitated by the three primitive colours alone which I have mentioned. † This was sufficient to suggest

Sometimes red, green and violet only exhibited by refraction.

The others compounded of these.

* This *dial* is simply a circle exhibiting the seven primitive colours conformably to the ideas of Newton. See Optics, Book I. Prob. 2. The author has explained the principles of the construction of this dial, and its leading properties, in his work.

† This proposition of three primitive colours is very different from

suggest to me the idea, that perhaps these three kinds of rays were all that really existed; a proposition that required to be examined with care proportioned to its importance. Accordingly I inquired into the probabilities that might be brought to support it, and compared it with all the phenomena of colour that occurred to me, and lastly I verified it by direct experiments.

The details of these I shall reserve for the last place, beginning with an account of the others.

This hypothesis not inconsistent with known phenomena.

I have already mentioned, that the supposition of three colours was not inconsistent with the formation of all the tints of the spectrum. Neither is it in contradiction with the unchangeableness of each tint by a second refraction: for if a red ray of a certain degree, for example, be found in the spectrum at the same place as a green of a certain degree, their combination will give a yellow of a particular tint; and as these two rays have the same refrangibility, a similar refractive power cannot again separate them. Accordingly, to have a spectrum in all points similar to that which really occurs, nothing more is necessary than to conceive it composed of three spectrums partly overlaying each other; one formed of red rays, differently refrangible, and of different tints; a second, trenching a little upon the first, and having only green rays, but a similar gradation of tints corresponding to their refrangibility; and lastly a third, exhibiting an analogous series of violet rays, and in like manner trenching upon the green. On this hypothesis, there will be no disruption of the whole image, whatever extent be given to it by refraction: besides, it accounts for seven colours separated by lines of demarcation, which no one yet has explained.

Explains the contiguity and distinction of the 7 colours.

To comprehend this, let us look at Fig. 1. Pl. II. which is constructed in the following manner: A right line is divided into seven parts, proportioned to the spaces of the seven colours in the spectrum, and marked by the initials of those colours. On each of the points of division I have

that formerly adopted; for the red, yellow and blue, have hitherto been so considered; while here they are the red, green, and violet, the exclusive existence of which is proved by the analysis of white light in several rays, as will be seen farther on.

erected

erected an ordinate, and afterward drawn the arbitrary inclined line *a d*, then *b g* cutting the former in *c*, and lastly *c h* cutting the preceding in *f*. I suppose, that the modifications of the red rays, on which their different refrangibility depends, are represented by the ordinates corresponding to the line *a d*: these quantities express nothing relative either to the velocity of the rays, or the magnitude of their particles; perhaps they may have a relation to their density, or to any other quality whatever that constitutes their difference. In Newton's system of seven classes of primitive colours, there are likewise red rays differently refrangible; this therefore is not a difficulty peculiar to the state of things I am examining. In like manner the ordinates of the line *g g* will be the modifications of the green; and those of the line *h e* the modifications of the violet. Hence it is evident, that the first division of colours from *a* to *b* will be red alone; that it will be followed by a mixture or combination of green and red from *b* to *c*, in which the quantity of the latter will predominate, and give orange; after which another mixture of red and green will proceed from *c* to *d*, in which the green will predominate more and more, forming yellow; then from *d* to *e* will be green alone; from *e* to *f* the mixture of green and violet that produces blue; from *f* to *g* the mixture producing indigo; and lastly from *g* to *h* pure violet.

Accounts for the brightness of yellow.

But another very striking property of the spectrum, which has not hitherto been explained, is the greater brightness of the yellow compared with the rest. This proceeds evidently in my figure from being the sum of the light of the red and of the green. In the blue too there is an augmentation of light by the union of the green and violet; but the effect is much less than in the preceding instance, both from the nature of those colours, and their extent, though there is some trace of it in the spectrum when properly displayed.

Blue next in degree of light, and why.

By this figure, however, I do not pretend to exhibit any thing more than what may possibly happen. For this reason I have limited the ordinates of each colour by a right line merely; for as the law of their progression is not known so that it is impossible to give the precise curve, I have adopted the simplest line as sufficient for my purpose.

The

Properties of the spectrum delineated on a circle.

The striking agreement of my hypothesis with the peculiarities of the spectrum excited me the more to apply it to the dial of colours. This coloured figure has such singular properties, that the mind cannot easily bend itself to them. How indeed can it conceive the existence of an infinite number of luminous rays, all different yet equally simple? How is it, that taken in pairs from the extremities of every diameter of the dial, that is from any two opposite points, they shall always form the same white? For instance, a certain red ray with a green gives white; an orange with a blue, the same; a violet with a yellow, still the same. What a strange similitude! How again are the seven distinct orders of the spectrum consistent with that insensible gradation of the tints of the dial recommended by Newton, and in fact necessary? Yet all these are so completely supported by experiment, that their reality cannot be questioned.

Solution of the problem.

Thus I had a problem to solve, the complicated data of which seemed at first not to promise a simple solution; yet, after various attempts, I attained my object, as will be seen.

First I considered, that both the nature and quantity of the red, green, and violet rays, which I suppose to be the sole elements of white light, are absolutely unknown. But I could likewise conceive them transformed into coloured matters of such intensity, or condensation, that the mixture of an equal quantity of each should produce exactly white.

In the second place I drew Fig. 2. This consists of three curves nearly circular and alike, described round the dial in the following manner. I first described three equal circles, having their centres in the radii drawn through the divisions of 60, 180, and 300 degrees; and the circumferences of which were targents to the dial at the divisions of 250, 360 and 120 degrees respectively I then modified each circumference by this law, that, on prolonging the diameters of the dial in every possible direction, the sum of the prolongations of every diameter to the new curve should be a constant quantity. It is easy to understand this second construction, by which it will appear, that the resulting curve differs in fact little from the circular circumference.

Thirdly,

Thirdly, I conceived, that all the prolongations of the radii of the dial to the red curve represented each a proportional quantity of my red matter mentioned in the paragraph before the preceding; so that this dial is surrounded by a red crescent to a certain point, whence it decreases according to a given law. We must likewise admit a green envelop, analogous to the preceding, and limited by the curve of that colour; and lastly a violet envelop, within the third curve.

This supposed, if for each point of the dial we make a mixture of colours corresponding to that point, we shall have a series of tints in imperceptible gradation from one to the other: which in tone, place, and every other respect, will be extremely analogous to the colours of the dial, that I had previously traced conformably to the ideas of Newton, and are such, that the union of two diametrically opposite to each other, will every where form a white identically the same.

This is a result which I offer as a farther probability greatly in favour of my hypothesis of three colours.

A little difference in the place of the colours.

It is true, the dial constructed by the first method differs a little from that by the last, as in this the purest red is somewhat nearer the place of the orange, and the violet nearer that of the indigo. But, beside that this difference is little in itself, it is supported by experience; for the relation of colours in general, and the progress of their absorption, appear to give some preference to the latter method.

Still I must repeat, that the observations I have here made are only to shew the possibility of the thing; the question can be decided only by the direct examination of the rays of light on the spectrum in its simplest state, and this remains for me to give.

Not easy to obtain a spectrum of the greatest simplicity; and few have done it since Newton.

As few have the means of procuring this very simple spectrum, and there is some difficulty in applying them, I shall enter into this subject somewhat at large. This I conceive to be the more necessary, as few appear to have repeated experiments of this kind since Newton, at least with due precision. Treatises on optics indeed do not mention this repetition formally, many philosophers having attempted

ed it without success, and others having persuaded themselves a little too hastily, that they had completely succeeded; as Abbe Nollet, for instance, whose name has been quoted as an authority.

I should not myself have had the means I long wished, but from the politeness and enlightened assistance of Mr. Trémery. Fortunately his study was provided with every thing necessary; but I shall first briefly describe the nature of the experiment, and the conditions indispensably requisite.

Nature of the experiment.

The business was to repeat the experiment, in which Newton obtained a well defined solar spectrum, the breadth of which, by concentrating the pencil of light, was reduced to $\frac{1}{70}$, or $\frac{1}{72}$ of its length; and which consequently exhibited the homogeneal rays incomparably more distinct from each other than in the common spectrum. Opt. Book I. Part 1. Exp. 11.

Conditions requisite to its success.

I have already hinted above, that the success depends, 1st. in operating on a pencil of light that is very small before it reaches the prism; 2dly. in producing by the prism a considerable dispersion of the coloured rays; and 3dly. in receiving their dissected image on a plane very distant from the point of the angle of dispersion.

Obstacles to it.

But these three conditions are not of themselves sufficient. It is almost impracticable to attain the desired object by their concurrence, when the rays arrive at first in parallel directions; still more if they arrive diverging, as they do when a pencil of light is admitted through a simple hole in the window shutter of a dark room; in which case the sensible diameter of the sun's disk must occasion a divergence of the pencil. There is only one circumstance favourable therefore, that in which the rays may be rendered convergent, without infringing the preceding conditions.

Newton's method, converging the pencil by a lens of little convexity.

The only method of doing this did not escape the sagacity of Newton. He effected it by placing at a considerable distance from the shutter, and but a little before the prism, a lens of a long focus, which by its position regulated the distance of the plane on which the spectrum was to be received. In this way, and by the assistance of some other precautions, he resolved this grand problem in optics.

The

The following is the manner in which we proceeded, and its results.

Difficulty of getting a good lens.

It is not easy to procure a single lens, that shall be capable of giving a focus of ten or eleven feet in the position in which Newton employed it; for several glasses of little curvature, that were lent me as fit for the purpose, were altogether incapable of effecting it. I then imagined, that I might succeed by placing near the shutter an object glass of short focus, to make the pencil very divergent beyond it; and placing at the same time at a sufficient distance, an excellent lens of Mr. Tremery's of five feet focus.

Resource of the author.

His apparatus described.

The effect answered our wishes, and in consequence we arranged our apparatus as follows: 1, on the outside of the windows, a plane metallic speculum, to reflect the solar image: 2, an object glass of 87 centimetres (33 inches) focus, distant from the speculum about 24 centimetres (9, 36 inches): 3, a diaphragm, pierced with a hole six millimetres in diameter (2.36 lines,) and at the distance of 11 centimetres (4. 3 inches) from the object glass, to introduce the pencil of light into the room: 4, a lens of 162 centimetres (5 feet, 3 inches focus, placed 32 centimetres (1 foot) from the object glass: 5, at 11 centimetres (4. 3 inches) from the lens a prism of very clear flint glass, with angles of 60°, covered with black paper on each side, except at the place left for the transmission of the rays; this prism being continued so as to be moveable in different directions, as occasion might require: 6, a board covered with white cloth, at the distance of 422 centimetres (13 feet, 8 inches) from the lens. All these were placed, kept, or brought into the proper directions, suited to their several purposes, and to the course of the sun. The place too was so contrived, as to be rendered pretty dark at pleasure. Having taken every possible care in arranging our apparatus, we were able to obtain every day, when it was fine weather, a very simple spectrum for several hours; which was quite sufficient for our various experiments, at some of which Messrs. Berthollet, the father and son, Mr. Laplace, and other gentlemen were present.

A very distinct spectrum 9¼ inches long

The spectrum was very distinctly bounded by two rectilinear, and perfectly parallel sides. Its length was a little more

Breadth 1-25th. more than 24 centimetres. (9. 36 inches) Its breadth was $\frac{1}{25}$ of its length, when the aperture in the diaphragm was 6 millimetres. (2. 34 lines) Sometimes this was reduced one half, and the contraction of the spectrum was proportional, the breadth then not being more than $\frac{1}{35}$; and lastly, by diminishing the aperture, it was reduced to $\frac{1}{85}$ of the length.

Colours bright and vivid. At a little distance it appeared triangular, the red forming the point.

As to the strength of the colours, they were vivid and bright as might be expected. The impression on the eye was such, on account of the narrowness of the image, that at a few decimetres (two thirds of a foot) from the cloth the spectrum appeared as two straight lines, forming a small angle, the apex of which was at the red extremity, and the base at the violet. On going nearer it appeared a single line. It was the same, if the spectrum were examined from a distance through a glass. This doubling or radiating of the image depends on the conformation of the eye, and is connected with some other phenomena, of which I may hereafter give an account.

The line of demarcation between the colours not very precise.

The distinction of the colours, and their separation into seven classes, was likewise one of the objects of our enquiry. Though the existence of this distinction was perceived, it must be confessed, that it was not easy to trace all their divisions. I made some attempts to effect it, the narrative of which I shall pass over for the present: merely observing, that Newton did not make his division on a spectrum thus narrowed, but on one much larger, obtained in the usual way without a lens. Opt. 1, part 2, prob. I.

More so in the common spectrum.

The green shortened;

the blue and violet lengthened,

from the nature of the glass.

Lastly I shall observe, that the green colour in our spectrum did not extend quite to the middle of its length, whence it followed, that the shades between the green and red were a little shortened, and those of the blue and violet proportionally elongated. These effects were owing no doubt to the nature of the flint glass, of which our prism was made. We had no opportunity of procuring common glass free from streaks. Having tried a hollow prism, formed of glasses joined together, and filled with water; the faces of the glasses occasioned duplications of the spectrum, which rendered it confused; so that we returned to our English flint glass, which, while perfectly void of colour, combined homogeneity of substance, and accuracy

of

of structure, with the finest polish; in short, it was to all appearance free from defect.

Experiments.

It now remains for me to speak of the particular experiments on the analysis of colours, which I had long planned.

Hypothesis, that blue is a compound of green and violet; yellow, of green and red. Consequences.

The reader may recollect, that I had suspected the blue to be merely the result of a combination of green and violet rays; and that in like manner the yellow proceeded only from a mixture of green and red. I reasoned then in this manner: on the supposition that in reality there existed no simple rays of blue, if we prevent the arrival of rays to that part of the spectrum, either by a substance that suffers only the green rays to pass, or by one that allows a passage to the violet only, we shall find beyond these substances only green or violet; otherwise, supposing the blue rays to be simple, they will traverse neither of the substances I have mentioned, as we shall find beyond them nothing but black. We may reason in a similar way with respect to the yellow, which must be subjected to the trial of a red substance and a green.

Requisites for the experiment.

Thus we must be furnished with three substances coloured in the requisite manner. For the violet I employed an ammoniacal solution of copper, in a phial with plane parallel surfaces: for the green a solution of muriat of copper, in a similar phial: and for the red, either wine of a good colour, or a tincture of cochineal. All these must be sufficiently concentrated, or they will transmit other rays, beside those we have in view. This concentration has the inconvenience of rendering the colour obscure, it is true, and this is some obstacle to their use; but it is the only way in which nature permits us to obtain simple colours, and we must be content with it.

Coloured glasses might be substituted for the red and green liquors; but with respect to the violet I could not procure any, on which I could depend.

Every thing being thus prepared, I made my experiments in concert with Mr. Tremery and Mr. Drappier.

Apparatus.

We had a screen, which we could place at will before the cloth opposite the place of the spectrum. In this screen was a small circular hole 3 or 4 millimetres (I. 17 or I. 56 line)

line) in diameter, by means of which we could allow a small coloured pencil to fall on the cloth, while all the rest was dark. We could easily ascertain, that this little pencil, taken successively from the different colours, was simplified as much as possible by refraction; for the circular spot, examined some distance with a prism, was not at all irregular.

We then passed through the small hole a pencil of very decided blue; and in this respect our latitude of choice was great, since in our spectrum the blue had an extent of more than 54 millimetres. (2 inches) The blue spot being well formed on the cloth, the green phial was placed before the hole; when the light of the spot was immediately much weakened, and its colour changed to green. On substituting the violet phial in the place of the green, the spot became violet. This experiment was repeated several times, that we might convince ourselves of the fact, and succeeded uniformly.

Blue light transmitted through a green medium was green; through violet, was violet.

The trial with the yellow succeeded in like manner; it was changed successively to red and green, according to the substance opposed to the rays.

Yellow changed to red and green.

Another day these experiments were repeated with some little alterations. When the small round image fell upon the cloth, we went behind to look at it; and found that it passed through, appearing on the back of the cloth, which was muslin well stiffened with starch. In this way we could make our experiments more conveniently, as we had only to cover our eye with a coloured substance, and look at the little spot through it. When the spot was formed by blue light, it appeared green, or violet, according to the substance interposed. Through a red substance no light was seen: a proof, that the preceding effect did not arise from white light mixed with blue. If it were viewed through an orange glass, the property of which is to absorb only the blue and violet rays, the spot appeared green; a proof that it was formed in reality by green and violet rays.

Experiment varied by viewing the image from behind the screen through a coloured substance put before the eye.

Blue light, as before.

Finally the yellow spot exhibited similar appearances; altogether invisible through a violet substance, it shewed itself green or red through substances of these colours.

Yellow, the same

Such are the results, that confirm my opinion of the elementary parts of light. Now let these be combined with the

Arguments for three primitive colours, red green, and violet.

the effects of absorption, which ultimately leaves only red, green, or violet rays; with the simple and natural explanation of the principal appearances of the spectrum, by means of three kinds of rays; with the happy manner in which these three kinds are applicable to the properties of the dial of colours, and remove its complication; and I think the whole will support my proposition. If it do not hence appear to the natural philosopher as a fact established beyond all question, at least he cannot refuse to consider it as already grounded on strong probabilities, and sufficiently interesting to merit a thorough investigation, which my occupations have prevented me from pursuing any farther.

Recapitulation.

Red and green produce yellow; green and violet, blue. Intermediate shades from different proportions.

Thus our system of colours appears to me reduced to these few data: three sorts of luminous rays, of a particular and unknown nature; red, green, and violet. Combined by twos, the red and green produce yellow; the green and violet, blue; the violet and red, purple: the three together produce white; and lastly, the intermediate shades are according to the proportional quantities of their elements.

Light analysed by refraction.

Bodies exercise a general action on all the rays of light, and a particular one relative to their peculiar nature. If the white pencil fall obliquely on the surface of a transparent body, the rays, as they penetrate it, deviate from their original direction, some more, others less, according to their nature. Here we have a true analysis of white light, in which its three simple elements may be found separate, as well as combined, in different proportions. It is thus that refraction exhibits a series of tints, which differ, in different bodies, both with respect to their general deflection, measured by its mean quantity, in the relative dispersion of the rays, and in the particular position of each colour.

Bodies coloured from absorbing particular rays.

If the affinity of the body for the rays of light, be such as to absorb some into its own substance, it will be coloured; and will exert a preferable or stronger action on certain sorts of rays. In a small mass, the body will first absorb these rays, to which it has a preferable affinity; and, if its action

action on the two simple kinds do not give a marked preponderance to one of them, it will be a mixed combination, that will first disappear. The mass of the body being gradually increased, the destruction of the rays will go by new mixtures, still progressively; the kind least acted upon will remain the last, and it will necessarily be one of these three, red, green, or violet; after which no more light will be transmitted. Such are the phenomena of absorption, and its different gradations.

Absorb others in succession, till by increasing their thickness they transmit none.

I shall give here one of the last results of my experiments, which might have created some confusion, had it been mixed with the preceding considerations, yet tends to confirm their principles.

I had an inclination to try, whether the light from a given part of the single spectrum, combined with that of another part chosen for the purpose, would produce white.

White produced by combining opposite points of the spectrum.

To carry this into execution, I placed before the image of the spectrum, received on the cloth a screen, by which one portion was concealed, and another left open. This screen, however, was perforated by a small hole, through which passed a ray of coloured light belonging to that part of the spectrum which was concealed. Lastly, this little coloured ray was received on a metallic speculum, placed between the screen and the cloth, and inclined so as to throw it on a given point of that part of the spectrum, which arrived freely on the cloth. Thus the colour arising from the mixture of two rays of light was observed.

I varied the trial of this apparatus on different points, the corresponding tints of which were diametrically opposite on the dial of colours. In several instances I did not obtain a white free from all tint of colour, because, a certain proportion in the quantity as well as quality of the elements is necessary: but having carried the little image almost to the limit of the green and blue, it gave a decided and bright white, when thrown on the extremity of the red.

Clearest white from the extremity of the red, and boundary of green and blue.

This very remarkable fact adds fresh support to my proposition respecting the compound state of the colour in certain parts of the spectrum, simplified to the utmost.

For,

For, if the combination of the three colours I have mentioned be necessary to produce white, as every thing tends to persuade us, we must admit the existence of violet in the greenish blue with which the experiment was made.

N. B. It may be necessary to add, in explanation of Fig. 2. Plate II. that the colours of the three circles are distinguished by dotted lines; the red by round and long points alternately; the green by one long point and two round; the violet by one long and three round.

VI.

Wooden Matches for Artillery to be used instead of Rope Match, or Port-Fires: read at the National Institute, April 1806. *By* C. L. Cadet*.

Rope or common match.

FOR several centuries rope match only was used for firing great guns, mortars, howitzers, and other pieces of artillery. This match, as is well known, is a rope of supple hemp, of a midling size, boiled for two hours in a bath of saltpetre, ashes, quicklime, and horse-dung. This rope, when dried, burns slowly to the end, in the manner of touchwood, and communicates its fire like red-hot coal. For use it is twisted round a staff called a port-match, and left to project near five inches beyond its end, this length burning an hour.

The inconveniences.

This match has several inconveniences. It requires constant attendance, since it must be unrolled from the staff every hour, or oftener; a tolerably heavy rain puts it out; it gives the artilleryman no light by night; and the end beyond the staff is not always steady, so that the gunner is slow in firing his piece. In these accounts its use is now confined to garrisons, except for carrying fire in the field, where for other purposes port-fires are employed.

Port-fires.

These port-fires are paper tubes, filled with a mixture of sulphur, saltpetre, and a very little neat-powder. This composition, the greater part of which is saltpetre, burns and melts with great activity, giving a vivid and bright

* *Annales de Chimie*, Sep. 1806, p. 314.

flame,

Their advantages.

Defects.

Particularly dangerous at sea.

flame, which quickly sets fire to the priming. In this respect they are far preferable to match, since they give light to the gunner, their fire is more vivid, and they are more easily guided; but these advantages are counterbalanced by dangers and defects. The saltpetre in these port-fires is never entirely burnt, but part runs out of the tube. When the materials are not well powdered, they are subject to spit, or throw out pieces of burning saltpetre to the distance of three or four feet, which may occasion serious accidents, particularly on board ships. I myself had my hair set on fire, and a hole burnt through both my coats, by a spark of this kind. In ships they are obliged to be kept in the middle of a tub of water on this account.

Wood impregnated with nitrate of copper, proposed by Borda and Proust.

These were the only means employed to fire pieces of artillery, when one of my correspondents at Madrid acquainted me, that Messrs. Borda and Proust had proposed to the Spanish government, to substitute instead of the cannon match, wooden rods impregnated with nitrate of copper. He added, that these rods burnt like touchwood, forming a pointed red coal; and that the trials with them succeeded perfectly, though they had not been adopted. I informed his excellency, the minister at war, of this new method; and he requested me to make the necessary experiments for ascertaining its utility, directing Mr. Lespagnol, a captain in the artillery, to assist me in the inquiry.

All wood not equally good for the purpose.

Nitrate of potash will not do.

My first idea was, that all kinds of wood could not be equally fit for the purpose; and that the difference of their porosity would occasion a difference in their combustibility. Before I tried the metallic nitrats, I took common saltpetre, and boiled several kinds of wood in a strong solution of it, which they imbibed in different proportions. This attempt did not succeed: the only wood that burnt quickly was the common cane, used for dusting clothes, or *rotang;* but its coal had no substance, the least blow breaking it off, and extinguishing it. I then got a joiner to make me some square rods, half a yard long, of oak, elm, ash, elder, birch, poplar, lime, and fir. I took two parcels of these, and boiled one in a solution

Different woods boiled with nitrates of copper and lead.

of

of nitrate of copper, the other in a solution of nitrate of lead. In each, the oak, elm, ash, and elder, were not saturated, and burnt in the usual manner: the others afforded me very good matches. But before I enter at large on their properties, I shall observe, that I conceive the nitrate of copper should be rejected, because it is too dear, it quickly corrodes the boilers, and its vapour is noxious. Accordingly I confined myself to the nitrate of lead; and I found after several trials, that it answered the purpose completely.

Oak, ash, elm, and elder, do not answer.

Birch, poplar, lime, and fir, do.

Nitrate of copper disadvantageous.

The wood that did best was that of the lime, birch, or poplar. To compare their properties, I weighed some rods both before and after boiling; I ascertained how much their weight was increased, and how long they continued burning; and I calculated how much of each a pound of nitrate of lead would saturate. The following table gives the proportions.

Lime, birch, and poplar best.

Their properties compared.

Name of the wood	Weight of a yard before the experiment. Grains.	Weight after. Grains.	Gained in weight. Grains.
Birch	888	1416	528
Poplar	516	936	420
Lime	888	1728	840

Name of the wood.	Length saturated by a pound of nitrate of lead. Yards.	Ft.	Inches.	Time each continued burning. Hours.
Birch	17	1		3
Poplar	21	2	8	2
Lime	10	2	9	3

From this comparative trial it follows, that the lime tree affords the best wood for matches for artillery; and with it I made the experiments desired by the minister, in presence of Mr. Lespagnol.

There are circumstances in which the service of the artillery requires light. Rods impregnated merely with nitrate of lead, produce a coal sufficient to discharge a cannon, but no light is afforded by them I conceived, that, if they were impregnated with oil of turpentine, they might

Light sometimes necessary.

Subsequent impregnation with oil of turpentine produced this.

might yield flame, without detriment to the action of the nitrate; and my hopes were realised, for rods thus prepared furnished both light and fire at pleasure. In this addition I found two other advantages: one, that of rendering the wooden match impervious to water; the other, that of facilitating the reduction of the lead, part of which I was apprehensive might be carried off in vapour, and injure the health of those who respired it.

Two other advantages in this.

The author's theory.

The theory of the process I adopted is simple; and it is easy to explain, why metallic nitrates succeed better than nitrate of potash. However dry the wood may be, it always retains a little of its water of vegetation or of composition, which is an obstacle to its proper combustion. By boiling the rods in a solution of nitrate of lead or of copper, which on account of its specific gravity requires a high temperature; this fluid dilates, softens, and penetrates the fibres of the wood, and expels their water of vegetation, which is replaced by that of crystallization. The nitrate then comes into immediate contact with the carbon of the wood, whence the rapidity of its combustion. The nitrate of potash does not answer so well, because, retaining much water of crystallization, its solution does not acquire so high a temperature: and, supposing it able to penetrate the wood as intimately, it carries into it too much water, for its combustion to be progressive and continual. A proof of this reasoning may be found in the composition of the two salts: nitrate of lead contains ·75 of its base, that of potash but ·49.

Lead easily reduced.

Its acetate converts rope into a match.

The rapid combustion of the wooden match is owing also to the facility, with which the salts of lead are reduced, when in contact with burning charcoal. If a hempen rope be boiled in a solution of acetate of lead, and afterward dried, it may be used as a match. It burns slowly like touchwood, and has a very bright coal. The oxide of lead, as the metal is reduced, gives out its oxigen to the carbon, and accelerates the combustion*.

Light woods absorb most salt.

On comparing the specific gravity of wood with its satu-

* We have a familiar instance of this in the popular experiment of burning a red wafer in the flame of a candle. Ed.

ration

ration by salts, we find, that the lighter the wood, the more saline matter it absorbs into its pores, or the interstices of its fibres. Hence it appears to me we may infer, that it contains less carbon than a heavier wood in a given bulk; and that its combustion will evolve less caloric, since the caloric emitted is in the ratio of the quantity of oxigen combined with the combustible. It seems to me, that we might class different kinds of wood, as to their combustibility, by their absorption of salts; and thus find which would be most advantageous to burn for domestic purposes, whether we would have a rapid combustion, or a stronger and more continued heat. These researches will form the subject of a particular work, which I purpose on all our forest trees.

This absorption a test of their combustibility.

The wooden matches, compared with port-fires, have the following advantages.

Comparison between the wooden match and port-fires.

The port-fire lasts but three or four minutes.

A match a yard long will burn three hours.

The port-fire is liable to break in the boxes.

The match is strong, and easily carried about.

The port-fire throws out dangerous sparks:

The match confines its fire to itself.

The port-fire costs from three pence to four pence half-penny:

The match costs but three half-pence or two-pence.

The last consideration is of great importance, since, from calculations made in the war-office, what would cost the state in the one case a thousand pounds, in the other would not come to more than seventy-five*.

Great saving.

As it was necessary to ascertain, whether these new matches would resist the rain, I had several burnt during long and heavy rains, and they were not extinguished till they were totally consumed; their combustion being a little retarded only.

The wood matches not extinguished by rain.

As the fabrication of these matches requires some care and precaution, I shall conclude this paper with a minute description of the process, agreeably to the request of

Precautions necessary in making them.

* According to the estimates just before given, the saving would be much greater than this on the lowest calculation. T.

his excellency the minister at war, for the instruction of the artificers employed in our arsenals.

Method of preparing the combustible wooden Matches for Artillery.—Shape of the Matches and Choice of Wood.

Shape of the match and choice of the wood.

The matches should be parallelipedons, half a yard long, and half an inch square. The best wood for them is that of the lime tree, or birch; but for want of these, poplar or fir may be used. Any white and soft wood might be taken, if necessary; but those above-mentioned are to be preferred.

Round inferior to square.

The shape might be supposed of no consequence: yet experience proves, that round matches do not furnish so good a fire as the square. The angles of the latter keep the coal in the centre burning vividly, and the match always terminates in a burning cone two inches long.

Drying the Wood.

The wood must be thoroughly dry.

Before the matches are saturated with nitrate of lead, the wood must be perfectly dry. For this purpose the wood should have been cut and stored at least a twelvemonth; and the matches, after they are shaped, be exposed for half a day to the heat of a stove at 30° (by what thermometer is not mentioned; probably 90°, or perhaps 100° Fh.) For want of a stove they may be put into a baker's oven, when the bread is drawn.

Furnaces and Boilers.

Boilers.

The fabrication of the matches requires two furnaces and two boilers. The shape of the boilers should be that of a fish-kettle, narrow, and three quarters of a yard long. Their size should be proportional to the quantity to be made at a time.

Furnaces.

The furnaces should be constructed so that the heat may act uniformly on every part of the bottom of the boiler.

First boiler.

The first boiler must be of copper, well tinned, and provided with a plate of the same metal, to press down the matches, and keep them immersed in the boiling solution.

Second Boiler.

The second boiler may be either of copper or of cast iron, placed on a sand bath, and having no

no direct communication with the fire. It should have a lid fitted to it very closely; and handles to lift it up when necessary.

Preparation of the Nitrate of Lead.

Preparation of the nitrate of lead.

To make this salt, nitric acid, or aqua fortis, must be saturated with red oxide of lead, or with litharge: but as it is necessary that the salt should be neutral, and have no excess either of acid or of base, some precautions in this operation are necessary. If the acid be too much concentrated, the salt will unite in a mass, crystallize confusedly, and contain a great deal of uncombined oxide. If too little oxide be used, the salt will be acidulous, and soon destroy the boilers. To obtain the mean term, 500 parts of litharge should be put into a vessel of glass or earthen ware, and on this should be poured 416 parts of nitric acid at 40°, (specific gravity we believe, 1.386) diluted with 128 parts of water; heat the mixture till the oxide is dissolved, filter, and evaporate to dryness. These proportions ought to produce 640 parts of nitrate of lead.

Proportions of the ingredients.

Bath of Nitrate of Lead.

Liquor for the boiling.

The nitrate of lead is very soluble in water, and the least possible quantity of liquid should be employed, that the bath, fully loaded, may acquire a temperature far beyond that of boiling water, and thus insinuate itself easily into the pores of the dilated wood. Accordingly, for every pound of nitrate, only a wine quart of water should be put into the boiler, or thereabout: but as different kinds of wood do not saturate themselves equally with the salt, their proportions must be studied. Experiment has shown, that to absorb a pound of nitrate of lead, requires near eleven yards of lime wood, 17½ of birch, and near 22 of poplar. The lime therefore, when saturated, is the most combustible.

Proportions of nitrate to different woods.

To render the saturation of the wood complete, six hours boiling are necessary, and hot water must be added, when the bath sinks so low as to let the salt fall to the bottom.

Time of boiling.

Second drying of the Matches.

When the matches are taken out of the boiler, they must

Second drying.

be carried to the stove, and made thoroughly dry, before they are put into the following bath.

Turpentine Bath.

Boiling in oil of turpentine.

Into the second boiler is to be put as much oil of turpentine, as will cover the matches to the depth of about an inch; and this is to be heated gently, till it begins to boil. But the moment it grows white and rises, the boiler must be covered, and quickly lifted off the sand bath, lest the oil should take fire. This boiling should be repeated two or three times, which will take about half an hour: the bath then is to be left to cool; the matches are to be taken out and wiped; and lastly they are to be dried in the stove, when they will be ready for use.

This paper was approved by the Institute, at its meeting on the 5th of May, on the report of Messrs. Carnot, Deyeux, and Guyton de Morveau.

VII.

Letter from a Correspondent on the Means of destroying the Insects which infest the Houses in large Towns.

To Mr. NICHOLSON,

SIR,

Introduction.

As you do not think it beneath the dignity of your Journal to descend to the disgusting, although often necesary business of considering the best method of destroying bugs and fleas, the following observations, suggested by your correspondent A in the last number of your Journal, are at your service, if you think they are worth insertion. I shall be very glad if they contribute to relieve your correspondent or any other of your readers from one of the "miseries of human life."

I am, Sir,

Yours,

W. W.

St. Mary-le-bone, May 6th, 1807.

Domestic insects infest dwellings in

Bugs are often intolerable pests in houses in large towns; more especially in inns, hotels, lodging-houses, &c. which

are

are exposed to continual importation of them upon clothes, packages, &c. When once they get into a house, although the numbers may be kept under by cleanliness, frequently taking down the bedsteads, and washing them with various kinds of poisonous washes; it is generally found that they cannot be eradicated. Their eggs or knits, or at least some of them resist the action of the poison, and after a time fresh swarms are produced, who live and multiply, especially in hot weather, in the apparently poisoned wood. Soon too they get into the wainscoat, skirting boards, or lath and plaster walls of the room, from whence they send forth fresh colonies as the former are destroyed.

instances where care and cleanliness are not sufficient remedies.

Six years ago last September I took my present dwelling house. The walls were repaired and white-washed. The bedsteads, one excepted, were all new, and that one was perfectly clean. Precautions were taken to prevent bugs being introduced in any old boxes, &c. of the servant.

Account of a house so infested.

Early in the spring all the bedsteads were, to my vexation and surprize, overrun with bugs—one in particular must have afforded habitation to several thousands.

They were all taken down and washed in soap and water, the ends were dipped in boiling water and then in a hot decoction of the cucumis colocynthin of Linnæn, or bitter apple as it is commonly called. In about six weeks it was necessary to take them down again. They were now washed in essential oil of turpentine, which kills this insect almost instantly; but appears to have no effect on its nit, and wholly evaporates in a few days. Therefore the joints were well brushed with a strong solution of oxymuriate of quicksilver, with which I hoped to render the wood poisonous and uninhabitable to them. But I soon found my hopes were vain.

Application of the usual remedies.

About this time I learned from a neighbour, that during the time of a gentleman who had lived in the house above twenty years, it had become overrun with this insect to a degree that appeared incredible? and that, until his death, he would not suffer his bedstead to be touched. The insects were sometimes seen crawling even upon the walls of his drawing room. After his decease millions were found upon his bed and chamber furniture.

I need

Remedies against the insects which infest our houses.

I need hardly say that this intelligence and my expence made me wish myself out of the house. But as I could not conveniently put my wish in execution, it was necessary to try to get rid of my co-tenants.

The bedsteads were taken down every three or four weeks during the summer and washed with decoction of hellibore, solution of arsenic, and various other poisonous washes. Generally some living bugs were found in them.

Early the next spring they were again taken down, and we had the mortification to find fresh colonies had taken possession, and were beginning to breed in the joints.

The skirting boards of the bed rooms were now removed, and in such rooms as were papered, all the loose paper was removed, and the rooms were well fumigated with oxymuriatic gas; after which the walls that were papered, were covered with paint; for fresh papering walls infested with this insect favors their increase.

The joints of the bedsteads were painted over with three coats of oxyde of lead mixed with linseed oil and a little rosin, so as to form a thick coat over the wood.

From this time, excepting a few stragglers who had got into the joint and died there; the bedstead continued quite free from the insect for two years, when a few were found in some parts where the paint had been abraded. The joints have since been painted over with a coat of thin paint once in two years. A precaution which I have used because the walls are not free from them. During very warm weather one or two are sometimes found upon the furniture; but such as get into the joints die.

Another bedstead, the joints of which happened to be painted with Spanish brown; bugs were found in the following year.

I wish this method may be found generally successful. The joints should be made easy and free from splinters, also all cracks and useless holes should be immediately filled up. If the insect has got into the walls the beds should stand six or seven inches from them.

On the subject of fleas I have no experience. Where from particular local situations cleanliness alone is insufficient to keep them away, the blankets may be dipped, and the floor,

floor washed in a decoction of the cucumis colocynthin, which I am told is poisonous to them. Your correspondent, if he pleases, may rub his body with it—it is perfectly safe; or he may put into his bed a bunch of fresh rue or savin, or perhaps of any other strong smelling herb.

VIII.

Theory of the Fabrication of Sulphuric Acid; read in the Class of Physical and Mathematical Sciences of the French National Institute, January the 20th, 1806, by Messrs. Desormes *and* Clement.*

Nitrate supposed to increase the temperature, afford oxigen, &c.

DIFFERENT opinions are entertained respecting the utility of nitrate of potash in the usual mode of fabricating sulphuric acid. Some believe, that the high temperature produced by its deflagration determines the formation of sulphuric acid; others imagine, that the nitrate affords the quantity of oxigen necessary to complete the combustion, which the atmospheric air has commenced: others again suppose, that water may be decomposed in the process, &c. We shall here attempt only the refutation of the first and second of these hypotheses, which appear at first sight the most probable.

Temperature not increased, because clay and water are present.

The first cannot be maintained, because, at the same time as nitrate of potash is added to the sulphur, clay and water are frequently mixed with it, each of which has the effect of diminishing the temperature; one by rendering the combustion more slow; the other by constantly absorbing a large quantity of the caloric evolved, to acquire the state of vapour. Besides, it is known, that sulphur burnt by itself, at a temperature of 1000° of the centigrade thermometer for instance (1832° F.) affords no trace of sulphuric acid.

Oxigen of the nitre too little to convert the sulphurous acid into sulphuric.

The other hypothesis, which does not appear so remote from the truth, is notwithstanding equally erroneous. It admits, that the oxigen extricated from the nitrate of potash is sufficient for the conversion of all the sulphureous

* An. de Chim. Vol. LIX. p. 329, Sept. 1806.

acid

acid gas produced into sulphuric acid; but the contrary to this is easily proved. The quantities of the elements that concur in this operation, or result from it, are not known with precision; yet those we shall assume may be considered as sufficiently near the truth to refute the second hypothesis.

Shewn by the proportions of their principles.

Nitrate of potash contains about 0.30 of nitric acid; which acid, according to Davy, contains 0.70 of oxigen. In this nitrate therefore there are $0.70 \div 0.30 = 0.21$ of oxigen. In sulphurous acid there are about 0.59 of sulphur, and 0.41 of oxigen; and in sulphuric acid 0.52 of sulphur, and 0.48 of oxigen. Now if we employ a very large receiver, or long continuance in a small one into which the air can enter, all the sulphur burnt with $\frac{1}{9}$ of its weight of nitrate of potash will be converted into sulphuric acid. Thus, if we operate with 90 parts of sulphur and 10 of nitrate of potash, we shall have $\frac{90 \times 100}{59} = 152$ of sulphurous acid, which will produce $\frac{90 \times 100}{52} = 173$ of sulphuric acid, and consequently require $173 - 152 = 21$ of oxigen. But the 10 parts of nitrate of potash, employed in this operation, could not give more than 2.1 of oxigen, or a tenth of the quantity necessary to saturate the acid. Some manufacturers carry the proportion of nitrate of potash to the sulphur as far as 0.2; but in this case, which is the most favourable to the hypothesis we controvert, the nitrate is but $\frac{10}{55}$ of what would suffice according to the proportions admitted. The nitre therefore does not serve to produce sulphuric acid, as has been supposed. If its oxigen be not sufficient to convert the sulphurous acid into sulphuric, still less can it suffice to saturate the sulphur with oxigen, without the assistance of the atmospheric air; and it is remarkable, that the acid contained in the sulphate of potash, the residuum of the combustion, contains more oxigen than the nitrate could furnish.

If any doubt of the solidity of this reasoning remain, on account of the uncertainty of the proportions of the substances operating, they will soon be dissipated, when the perspicuousness of the new theory is contrasted with these vague opinions.

When

When we attentively observe the burning of the ordinary mixture of sulphur, nitrate of potash, and wet clay, we perceive, that the nitric acid is not completely decomposed, and that a great deal of nitrous acid gas passes into the leaden chamber with the sulphurous acid. Its colour renders it very visible, and it is a fact that cannot be questioned.

In the ordinary process nitric oxide evolved.

This observation affords a key to the true theory; and in following up its consequences we find the production of sulphuric acid clearly explained.

This the key to the true theory.

We are certain, that the combustion extricates a mixture of nitrous acid gas, and sulphurous acid, with water in vapour, and nitrogen gas from the atmospheric air. We may suppose too, that a portion of oxigen has escaped the action of the sulphur. This supposition, which has nothing in it that is not extremely probable, is the only thing on which any doubt can be entertained. Now, from an experiment made purposely to ascertain this, the sulphurous acid gas and nitrous acid gas cannot exist in contact, without the latter being decomposed, and converting the former into sulphuric acid; this then will take place, when such a mixture of the two gases takes place in the leaden chamber. Being then at a distance from the place of the combustion, this mixture finds a lower temperature, which occasions the condensation of part of the vapour; the rain thus formed carries with it the sulphuric acid produced, and affords a vacuum to the different substances that remain; these precipitate themselves into it in eddies, and present to each other a thousand points of contact that favour the action of their affinities.

Recital of the facts.

Nitric oxide converts sulphurous acid into sulphuric.

After the first production of sulphuric acid, there remain nitrous oxide gas, sulphurous acid, atmospheric air deprived of part of its oxigen. The nitrous oxide necessarily converts itself into nitrous acid, which will be again decomposed to the profit of a second portion of sulphurous acid; and this will go on till all the nitrous acid or atmospheric oxigen, or both, are exhausted.

The nitrous oxide remaining takes oxigen from the air, becomes nitric oxide, and then yields its oxigen to the sulphurous acid.

The first productions of sulphuric acid must be the most copious and rapid, because the condensation of the aqueous vapour produces a great commotion in the mixture of the different gases; and because too the abundance of the oxigen

The sulphuric acid produced most copiously at first.

igen and sulphurous acid render the contact more probable, while, as they become less in quantity, the nitrogen, which continues the same, renders their approximation more difficult.

Residuum, nitrogen, nitrate or nitric oxide, and perhaps oxigen.

After the whole of the sulphurous acid is converted into sulphuric, the substances that remain are a great deal of nitrogen, nitrous oxide, or nitrous acid gas, if there were at first more oxigen than the sulphurous acid required; and perhaps an excess of oxigen more than sufficient to saturate the sulphurous and nitrous acids.

Quantity of oxide of nitrogen remaining.

What is of importance to be observed is the base of the nitric acid, the quantity of which cannot have varied, and which ought to be as much after the production of all the sulphuric acid as at its extrication from the nitrate of potash. This quantity of nitrous oxide, or nitrous acid, is probably a little less than the nitrate could have produced, because in the combustion the temperature may have been raised too high, and then the complete decomposition of a small portion of nitric acid takes place. We say a small portion, because experience has shewn the advantage of keeping the temperature very low by a suitable quantity of moisture.

The nitric acid acts as an intermedium.

Thus the nitric acid is only the instrument of the complete oxigenation of the sulphur; it is its base, the nitrous acid, that takes oxigen from the atmospheric air, to present it to the sulphuric acid in a state suitable to it.

Water indirectly necessary.

We see that water is not directly necessary to the production of sulphuric acid; its combination with what is formed merely effects the extrication of the nitrous acids that must have combined with it. This gas, thus set free, proceeds afresh to seek oxigen from the atmospheric air contained in the receiver, to unite it again with the sulphurous acid. The aqueous vapour has at the same time the double advantage of producing a great commotion in the remaining gases, and of producing this evolution of nitrous acid gas; accordingly its utility has been perceived, and a quantity is introduced, by the exhalations from the hearth, beside that arising from the humidity of the mixture.

Thus setting out from the existence of nitrous acid and sulphurous acid gases, we have followed the metamorphoses these two bodies undergo, taking for our ground-work facts

facts well ascertained; and have admitted only one single supposition, that of the existence of a portion of oxigen still free after the passage of the air over the sulphur. If this supposition should appear doubtful, at least it will cease to be so, when we have shewn by experiment, that, admitting it, every thing takes place as we had conjectured.

The whole process may be seen in a glass vessel.

By mixing in a transparent vessel the different substances we have considered as essential to the operation, we can see whether the succession of combinations be such as we had conceived. And it may be verified by putting into a glass body sulphurous acid gas, atmospheric air, and nitrous oxide gas in small quantity, for instance $\frac{1}{20}$ the weight of the sulphureous acid; for we see the oxide grow red, and diffuse itself throughout the whole space; then clouds of white fumes roll across the vessel, and deposit themselves in shining stellated crystals against its sides. These dense whirls of sulphuric are succeeded by an appearance of clearness; and, if at this instant a little water be admitted, the crystals of acid dissolve with great heat; the nitrous oxide gas, again becoming free, changes afresh to a red vapour; and the same phenomena re-commence, till all the atmospheric oxigen is consumed, or all the sulphurous acid burnt.

The remaining gases are precisely those we mentioned in our conjectures; for the colour of the nitrous acid appears with almost all its first intensity; and after the operation is completed, there is no more smell of sulphurous acid, but a great deal of nitrogen, and of oily sulphurous acid on the sides of the glass.

Too much water would produce nitric acid.

If in this combustion of the sulphurous acid there were too much contact between the gases and the water added, either by great agitation of a little, or by the presence of a large quantity, the operation would be very slow and incomplete, because liquid nitric acid would be formed, which retaining its state, would have very little action on the gas to be organized.*

The experiment does not always succeed.

* It sometimes happens, that the decomposition of the nitrous acid gas is carried so far as to the state of an oxidule of nitrogen; this too appears to arise from too great action of the water on this gas. Messrs. Berthollet and Guyton have ascribed to this the miscarriage of the experiment, when the contact of water is too great.

This

This experiment, the only one of the kind, leaves no doubt respecting the theory of the fabrication of sulphuric acid, which we have here offered, and which is only a simple exhibition of the facts. If the chain of ideas to be adopted, in order to arrive at the process actually pursued, and the few analogies this operation has to all that we know, be considered, it will appear very fortunate, that chance alone, in some sort led to the discovery; and that we were then put in possession, without knowing it, of the only process perhaps capable of furnishing sulphuric acid by the combustion of sulphur in the air.

The discovery of the process owing to chance.

This theory, affording us the means of improving our knowledge of the proportion of the elements of sulphurous and sulphuric acid, gives us some hope of discovering the same mode of action in other chemical operations, perhaps ill understood; it likewise permits us to add some improvements to the present mode from just principles; as the extent and form of the leaden chambers, and the management of the fire, must be necessarily influenced by this hypothesis; but its first benefit will be a saving of almost the whole of the nitrate of potash.

Advantages to be expected from this theory.

P. S. In the meeting of the 1st. of September, 1806, the Physical and Mathematical Class of the National Institute ordered this paper to be printed in the collection of those of learned contributors.

IX.

Facts toward a History of Cobalt and of Nickel, by Mr. Proust; *abridged by Mr.* Chevreuil.*

Action of acids on cobalt.

Sulphuric, muriatic, and nitric acids, oxide cobalt in the same manner. With the first and second hidrogen is evolved.

Sulphates.

Two sulphates.

Of these there are two, one simple, the other a triple salt, with the addition of potash or ammonia.

* Annales de Chimie, Vol. LX. p. 260, December, 1806.

1. The

1. The simple sulphate has a taste slightly pungent, and a little bitter, with something metallic. Its crystals, which are of no great bulk, are sections of irregular octaedra heaped together, of a gooseberry red colour, and unalterable in the air. By distillation they lose 42 *per cent.* of water, and are rendered rose-coloured and opake. In this state they can endure a red heat without being decomposed, except in the points that touch the retort. Simple sulphate.

2. When sulphate of potash is mixed with the preceding sulphate, we obtain more bulky crystals, which are rhomboidal cubes. This triple salt is less soluble than the simple sulphate, and loses only 26 *per cent.* of water by distillation. Triple sulphate

Carbonate.

Carbonate of potash produces 40 or 42 hundredth parts of carbonate of cobalt with the simple sulphate. An excess of alkali dissolves a great part of the precipitate. Boiling, or cold water, decomposes this solution. Carbonate.

Oxide at a Minimum.

A hundred parts of the carbonate, after the separation of the water and carbonic acid, leave 60 or 62 of greenish grey oxide. To have it very pure, the retort must be as full as possible, and heated gradually. Without these precautions it will be mixed with oxide at a *maximum*, which yields oxigen gas with muriatic acid, while that which is pure does not yield an atom. Greenish grey oxide.

The grey oxide dissolves with heat in nitric acid, without yielding nitrous gas. Heated in contact with air it immediately becomes black; an oxide of which part is carried to the maximum is easily detected by the application of a weak acid, which dissolves only the oxide at a minimum. Ammonia produces the same separation, as Thenard observed. Heated in the air becomes black, and insoluble.

Oxide by Precipitation.

1. A few drops of nitrate of cobalt dropped into *boiling water* alkalized with potash give a blue precipitate, which ultimately becomes of a rose colour, if the boiling be continued. In this case a hidrat is formed. Nitrate precipitated by potash and converted into a hidrat by boiling.

2. If

Without heat the precipitate is green.

2. If *cold alkalized water* be employed, the blue precipitate is formed likewise; but, instead of constituting a hidrat, it passes to green, without the contact of air being capable of obscuring its tint, which it retains after it is dried.

Changed to a grey by boiling.

3. If this green precipitate, when fresh made, be boiled in water alkalized with potash, it becomes of a reddish grey, and changes no further.

Action of acids on the precipitates.

Very weak acids, as vinegar for instance, totally dissolve the first precipitate. Applied to the other two, they separate from it black oxide. Lastly, the blue oxide yields no gas with muriatic acid, but the green does.

Green oxide a compound of the blue and black.

Hence we must conclude, that the blue oxide oxigenizes itself at the expence of the air contained in cold liquors, and that the green oxide is a mixture of blue oxide and black oxide. Mr. Proust however thinks, that something more than simple mixture takes place; for blue and black would not produce that grass green colour, which distinguishes it from every other oxide. A true combination alone could form a colour different from that of the mixture of its component parts, and prevent the action of the air from raising to a maximum the portion of blue oxide, which makes a part of the green precipitate. To oxide this precipitate completely, it must be dried with the assistance of heat, as Thenard shewed.

The reddish grey precipitate of the third experiment is a mixture of hidrat and black oxide.

Maximum oxide only soluble.

The oxide at a minimum only is capable of combining with acids. The green oxide is never obtained from any solution, and cannot become the base of any saline combination.

Ammonia and Oxide of Cobalt.

Dissolves with difficulty in ammonia,

If the grey oxide be enclosed with ammonia in a well stopped phial, it imparts to it a slight rose colour, which does not become deeper, however long it may be kept. This oxide is consequently very difficultly soluble in ammonia.—

readily in its carbonate.

But if the phial be left open, the ammonia becomes coloured very quickly, because it attracts carbonic acid from the air. This solution may be effected in a very little time, by placing the phial in a large jar containing a carbonate.

If

If the ammonia be merely saturated with carbonic acid, the liquid will be a solution of oxide of cobalt in carbonat of ammonia: but if we continue to pass corbonic acid into this solution, we obtain a solution of carbonat of cobalt in carbonat of ammonia. This solution, kept in a full bottle corked, deposits crystals of metallic carbonate; it likewise lets fall a part on the addition of water; but an excess of ammonia redissolves this precipitate.

This a solution of the oxide, or of carbonat.

This solution may be made very quickly, by throwing carbonat of cobalt into carbonat of ammonia.

Made directly.

If pure ammonia be poured on carbonat of cobalt with excess of acid, what occurs is very different. The carbonat of cobalt separates into two parts; one gives out its acid to the ammonia, and becomes a hidrat, which falls to the bottom of the vessel; while the portion not decomposed dissolves in the carbonat of ammonia.

Ammonia with carbonat of cobalt,

Thus we have two kinds of ammoniacal solutions of cobalt; and there is a third, which Taffaret observed, but which has been hitherto little noticed. This is obtained by putting well washed hidrat, or blue oxide, into a phial full of ammonia, and closely stopped. A solution will take place in the course of four and twenty hours. This is red, like the preceding; but differs from them in this, that, if it be poured in a very slender stream into boiling water, blue oxide will immediately be precipitated; if into cold water, green oxide will be obtained. If ammonia dissolve hidrat of cobalt, or blue oxide fresh made, more readily than the gray oxide, it is because they are in a state of extreme division.

and with hidrat, or blue oxide.

Distillation of ammoniacal solutions.

When carbonated solution of cobalt are distilled, carbonate of ammonia passes over, and at length the liquor lets fall an oxide, which is at first of a dirty green, but which afterwards becomes black. This is a mixture of the gray and black oxides.

The carbonated solutions distilled leave the cobalt more oxided.

How is this superoxidation effected? The author reports the facts, but does not endeavour to explain them, when data are wanting.

Hidrat of Cobalt.

Hidrat of cobalt,

Crystals of sulphat or nitrat of cobalt, thrown into a bottle filled with a solution of potash, and immediately corked up, are decomposed. A blue precipitate is formed, which changes to a violet, and afterwards to a rose colour, becoming a hidrat.

decomposed by potash,

If the hidrat of cobalt be boiled with potash, this dissolves some oxide, and acquires a fine blue colour. This solution is decomposed by the addition of water. By exposure to the air the oxide becomes black, and falls down.

and dissolved in its carbonat.

Hidrat fresh made dissolves without heat in carbonat of potash, and tinges it red. The oxide does not dissolve in it.

Characters of the hidrat.

The hidrat of cobalt is of a rosy feuillemorte colour. Acids dissolve it with heat, and without effervescence.

The hidrat is not decomposed by boiling either in pure or in alkalized water. Heat expels from it 20 or 21 of water, and reduces it to very pure gray oxide.

It does not keep well under water; when it is exposed to the contact of air, it grows black. Dry hidrat keeps better, but it attacts carbonic acid.

Dissolves in ammonia as it forms from the sulphat.

When crystals of sulphat of cobalt are thrown into a phial full of ammonia, which is immediately closed, they yield a blue precipitate, which does not become rose coloured, as in potash. Mr. Proust affirms, that the hidrat is formed, but that as fast as it is produced it dissolves in the ammonia; so that it is the hidrat that colours the solution, and not the simple oxide.

Estimation of the quantity of oxigen in the oxide at a minimum.

Gray oxide contains 16·5 per cent. of oxigen.

A hundred parts of gray oxide, reduced with the requisite precautions in a closed crucible, afforded 83·5 of metallic grains. One hundred parts of the metal therefore appear to absorb nineteen of oxigen, to become oxide at a minimum.

Oxide at a maximum.

Black oxide 20 per cent.

If a nitric solution of cobalt be distilled, black incrustations will

will be deposited on the sides of the retort, nitrous gas will be evolved, and the residuum obtained will be black oxide. The quantity will be in the proportion of 125 or 126 parts to 100 of the metal in the solution. Hence we may infer, that the maximum of the oxidation of cobalt is between 25 or 26 of oxigen to a hundred parts of the metal.

This oxide is not soluble either in the nitric or sulphuric acid, without losing that portion of oxigen, which converts it to the state of a maximum. Insoluble in acids without losing oxigen,

With muriatic acid it gives out oxigen gas.

It is insoluble both in ammonia and in potash. and in alkalies.

The black oxide, heated for half an hour at the bottom of a retort, returns to the state of gray oxide by parting with oxigen; and it is then capable of giving a blue tint to vitrifiable substances. Heat converts it to gray oxide.

Messrs. Proust and Thalaker have met with the black oxide at Pavias, a day's journey from Valentia. It is found likewise in those ores of cobalt which are termed irtreats, or black ores. Native.

The corbonat and hidrat of cobalt are changed into black oxide by oxigenized muriatic acid. From carbonat and hidrat.

The nitrous and sulphurous acids dissolve the black oxide, forming with it nitrat and sulphat at a minimum. Soluble in nitrous and sulphurous acids.

Muriat of Cobalt.

The gray oxide disolves with heat in muriatic acid of 15°. The solution, whether hot or cold, is of a deep blue: it crystallizes easily, and the crystals are blue: this is the anhidrous muriat. As soon as it has absorbed moisture, it becomes red. Blue, or anhidrous muriat, and red.

Muriatic acid of 15° yields much gas with the black oxide. This solution is green as long as it retains any gas; but as soon as it has lost it, it becomes blue. The blue traces of muriat of cobalt dried on paper are anhidrous muriatic. When they are green, it is because the salt still contains muriat of nickel, which gives a yellow tinge, and thus forms green with the blue. Green muriat contaminated with nickel.

Its distillation.

Urged to redness in a luted retort, it is decomposed only in Heated, what touches the glass is decom-

posed, the rest sublimes.

the parts that touch the glass. The products then are muriatic acid gas mingled with oxigenized acid. The glass becomes tinged with blue. The muriat that is not decomposed sublimes, after having melted, in flowers of a gridelin colour. These flowers have acquired a kind of condensation, which renders them insoluble in water for twelve hours at least: but at length they afford a solution of ordinary muriat.

Arsenit and Arseniat.

Arsenit, how prepared.

The Arsenit of cobalt is prepared by pouring a very dilute solution of cobalt into a solution of arsenit of potash. A rosy precipitate is formed, which retains this colour after desiccation.

Character of the Arsenit.

Its characters.

1. Heated in a tube closed at one end it is decomposed; the oxide of arsenic sublimes; and the glass is tinged blue.

2. The nitric acid dissolves it, and nitrous gas is evoled.

3. Its solution in muriatic acid is decomposed by sulphuretted hidrogen, which precipitates orpiment.

4. Caustic potash, assisted by heat, separates the blue oxide.

Arseniat.

Arseniat.

This is obtained by employing arseniat of potash, instead of arsenit. The precipitate is rose-coloured like the arsenit.

Its Characters.

Its characters.

1. Heated in a tube it yields no sublimate, and becomes violet-coloured, without tinging the glass.

2. Nitric acid dissolves it without giving out nitrous gas,

3. Its muriatic solution does not become turbid by the addition of sulphuretted hidrogen in less than two hours after they are mixed.

4. Caustic potash separates blue oxide and combines with the acid.

Native.

The rosy efflorescences found or minerals containing cobalt consist

consist of arseniat. Mr. Proust found arseniat only in the interior parts of some fragments.

Hidrosulphuretted oxide. Sulphuret of Cobalt.

The gray oxide, the hidrat, and the carbonat, take sulphuretted hidrogen from water, and become hidrosulphuretted oxide. This is not soluble in ammonia. By distillation it gives out water and sulphurous acid, and the residuum is a sulphuret. Hidrosulphurets, and sulphuret.

The oxides heated with sulphur are converted into sulphurets.

One hundred parts of cobalt absorb forty of sulphur; though Mr. Proust has some doubts respecting this proportion.

Facts toward a history of Nickel.

Nitrat of Nickel.

A hundred parts of the metal dissolved in nitric acid, and distilled till they are completely decomposed, leave 125 or 126 of greenish gray oxide at a minimum. Nitric acid can not convert this oxide to the maximum state. Nitrat of nickel.

To ascertain the purity of the oxide of nickel, it must be dissolved in muriatic acid, and exposed to the action of heat. If it contain a little oxide of cobalt, oxigenized muriatic acid gas will be evolved; if it be pure, more will be given out. Test of its purity.

The gray oxide dissolves in all the acids, and affords the same solutions as the metal itself.

Nitrat at a Minimum.

If nitrat of nickel be distilled with the same precautions as nitrat of copper, we obtain, as with the latter, a nitrat with excess of base, which is insoluble in water. Of this nitrat 142 parts are afforded by 100 of nickel; so that, if we substract the 25 parts of oxigen, which the metal has absorbed, we have 17 parts of acid combined with the oxide. Minimum nitrat, nickel 70·4, oxigen 17·6, acid 12.

Nitrate

Nitrat at a maximum.

Maximum, nickel 20, oxigen 5, acid 55, water 20.

100 parts of dry nitrat of nickel yielded by distillation 20 of water and 25 of oxide; consequently contained 55 of acid. These proportions are not exact, as the last portion of water were acidulous.

Muriat of Nickel.

Muriat.

This is a very deliquescent, apple-green, granulous crystallization.

Its traces on paper, when dry, are yellow.

Contains 55. water.

This muriat loses 55 of water. What remains is a yellow anhidrous muriat, changing green again in the air, by absorbing water.

Anhidrous muriat.

If the anhidrous muriat be placed on the fire in a glass retort, and the heat strongly urged, it does not melt; the parts that touch the glass are decomposed; muriatic and oxigenized muriatic acid are evolved; and the undecomposed salt sublimes in the form of flowers like mother-of-pearl, of a golden yellow colour. These flowers absorb moisture, and become green in the course of a couple of days. The muriatic acid dissolves them with difficulty.

Oxide 33·5, water 55, muriatic acid 11·5.

100 parts of muriat of nickel, decomposed by carbonat of potash, produced 61 or 62 of carbonat, which infers 33 or 34 of oxide.

Sulphat of Nickel.

Two sulphats.

This is either simple, or combined with potash. The first crystallizes in hexaedral pressure, terminated by an irregular pyramid: the second, in rhomboids.

Simple sulphat.

The simple sulphat loses 46 of water; and the anhidrous residuum becomes green again by absorbing moisture, kept at a strong red heat for an hour in a luted retort it is reduced in part to the state of sulphat with excess of base. The water takes up that which has lost none of its acid.

100 parts of this sulphat afforded 64 of a light green carbonat.

Triple sulphat.

The triple salt with potash loses 24 of water. The residuum

duum comports itself like that of the simple sulphat. 100 parts of this salt afford but 27 or 28 per cent. of carbonat.

Both these sulphats of nickel are transparent, of a fine emerald green, and unalterable in the air. Mr. Proust is of opinion that the sulphat of potash unites with that of nickel in a constant proportion.

Extraction of Nickel in the large way.

Method of obtaining nickel pure.

Suppose we have an abundant solution of the ore, first calcined, and afterward vitriolized with the residuums of ether. The business is, to separate the nickel from iron, copper, arsenic, bismuth, and cobalt. The iron is at a maximum of oxidation, and in this state has little affinity with acids. It may be precipitated then in the state of arseniat, by means of potash gradually added. Ammonia, or a prussiat, will afterwrad indicate, whether it be entirely thrown down.

Iron precipitated by potash.

Copper, bismuth, and arsenic, by sulphuretted hidrogen.

Into the filtered solution let a stream of sulphurated hidrogen be passed the copper, bis muth, and all the arsenic, will be precipitated in the state of sulphuret.

Sulphat of nickel from that of cobalt, by crystallization,

When the sulphuretted hidrogen no longer occasions any precipitate, the liquor is to be evaporated for crystallization. The triple sulphat of nickel and potash, being less soluble than that of cobalt, crystallizes first. By repeating the crystallizations the two salts are separated: but the last portions of the salt of nickel will be contaminated with some sulphat of cobalt, from which they may be freed by washing in cold water.

and ablution,

in silver vessels.

All these crystallizations require a basis of fine silver, if we would have the operations go on smoothly.

Test of its purity.

A salt of nickel is known to be pure, when the precipitate, dissolved in ammonia, quits this solvent, without our finding any cobalt at the end.

Sufficient potash must be used.

When we precipitate a sulphat of nickel, we must not be too sparing of potash, otherwise we shall be in danger of precipitating sulphat with excess of base, which would affect the purity of the precipitate.

Carbonat of Nickel.

Carbonat of nickel.

A hundred parts, heated in a retort, five 54 or 55 of greenish

ish gray oxide at a minimum. When heated in contact with the air, the oxide is black.

The minimum oxide is converted into carbonat by exposure to the air.

Hidrat of Nickel.

Hidrat. All the salts of nickel, thrown into a boiling solution of potash, are converted into a green hidrat. Boiling does not alter its colour. Potash does not dissolve either the hidrat or oxide of nickel.

The hidrat is reduced to gray oxide by heating.

In this state in all its salts. In the saline combinations the oxide is in the state of hidrat. Alkalis precipitate it in this state.

Maximum oxide of Nickel.

Oxide at a maximum. The carbonat and hidrat are both oxided to a maximum by the action of oxigenized muriatic acid. The gray oxide is more difficult to oxide.

Black. The dry oxide of nickel at a maximum is black. When solid its fracture is vitreous.

Oxigen expelled by ammonia. This oxide kept in ammonia gives out bubbles, returns to the state of gray oxide, and dissolves in the alkali.

With muriatic acid at 15° it yields a considerable quantity of oxigenized acid. The solution is greenish yellow, and crystals form in it by cooling,

Oxides reduced. The oxides of nickel are reduced like that of cobalt. The metal is obtained pretty easily in a button, in which it differs from cobalt, this affording only large grains.

Sulphuret. This metal appears to have taken up a surcharge of sulphur of 46 per cent.; but Mr. Proust entertains some doubt of the accuracy of this proportion.

Arsenit and Arseniat.

Arsenit. These are made like those of cobalt, and are of a fine apple-green colour.

The arsenit, heated in a tube, loses its colour with its water, gives out white oxide, and changes to an olive green. To abstract all the arsenic the contact of charcoal is requisite.

Heated

Heated under charcoal in a spoon of platina, the arsenic is quickly dissipated, and oxide at a minimum remains.

The arseniat, heated in a tube, loses its colour with its water; becomes transparent and of a hyacinth red; but if the heat be carried to redness it turns of a pale yellow, and remains unalterable. Arseniat.

In the spoon the arseniat turns white, and grows red hot without melting, or emitting the least arsenical fume. To decompose it an obscure flame is required.

Recapitulation.

From the preceding facts, and others which he has given in different memoirs, Mr. Proust concludes, that cobalt, nickel, and most of the well known metals, have but two very decided degrees of oxidation. He does not mean to say, that a metal can absorb oxigen in too proportions only: he only asserts, that it is too soon to admit all the oxides which have been mentioned by chemists, and of which neither the quantity of oxigen, nor the combinations they are capable of forming with acids, can be considered as determined; and that colour is not a character sufficient to constitute a distinction. Most metals have but two oxides, at least that are yet known,

There are but too metals, that have hitherto afforded him more than two oxides. These are tin and lead. However, the quantity of oxigen in that oxide of tin, which constitutes the base of mosaic gold, is not yet known; or that of the oxide of nitrat of lead made by boiling with plates of this metal. The only excepting tin and lead.

It seems, that the different oxides of the same metal can mutually dissolve each other, and form true combinations. Thus the green oxide of cobalt is a combination of the blue and black oxides. Oxides of the same metal continue chemically.

Is not minimum a combination of the brown oxide of lead and oxide at 9 per cent. in a similar manner? Minimum perhaps a compound.

Finally, all the magnetic ores of iron, and magnetic sands are mixtures or combinations of this order. If this were not the case what would prevent the minimum oxide from being raised to a maximum of oxidation? The oxide of a gun-barrel that has been used for decomposing water is likewise in a similar state, it is composed of two oxides. Magnetic iron ores such compounds.

XI.

Facts toward a History of the Gallic Acid. By Bouillon Lagrange.*

I HAVE had the honour of submitting to the class the result of my experiments on tannin; I now lay before it some facts respecting the gallic acid, which I had announced as forming the second part of my memoir.

History of the gallic acid.

Of all the vegetable acids, the gallic may be considered as most interesting, and accordingly it has been a subject of inquiry to many chemists. Macquer, Monnet, Lewis, Cartheuser, and Gioanetti, pointed out the manner, in which solutions of iron are acted upon by substances called astringent. The academicians of Dijon were the first, who observed the presence of an acid in those subtances; and in 1772 they shewed, that the distilled products of nutgalls blackened the solution of sulphat of iron, and that an infusion of them reddened the tincture of bitmus. These particulars afforded only a general proof of the acid nature of the principle contained in galls; offering no means of extracting this acid, and obtaining it separate, for which we are indebted to Scheele. His process was published in 1780. A few years after, in 1793, Mr. Deyeux discovered that this acid might be obtained by sublimation. Messrs. Berthollet and Proust afterward added much by their researches to our knowledge of the properties of this acid; so that it might be considered among the best known of all that the vegetable kingdom produces.

Scheele first extracted it. Deyeux sublimed it.

Several foreign chemists too, within these few years, have given processes for extracting and purifying this acid: but none of them, except Richter's, can come in competition with Scheele's. Among the many experiments, that have been made on this subject, there is one, which I have neither seen refuted nor quoted in the papers published on the gallic acid.

Some facts by Bartholdi unnoticed.

In a letter from Mr. G. C. Bartholdi to Mr. Berthollet, dated 1792, there are some facts, that might have claimed the attention of chemists.

* Annales de Chimie, Vol. LX. p. 156, Nov. 1806.

Mr. Bartholdi first points out a process for obtaining pure gallic acid: he afterward treats this acid with metallic oxides; and he says he has demonstrated, that all substances, which yield oxigen to the gallic acid, give it a brown colour; and that, in this process, it is the acid itself, which, being charred, forms by a slight combustion the colouring matter.

Oxiding substances brown the gallic acid by slightly charring it.

To show this, he boiled red oxide of mercury for half an hour in a solution of gallic acid, which assumed a blackish hue. In the residuum he found fluid mercury, mixed with a coally powder: he afterward saturated the liqour with carbonate of potash and soda, and the salts thus produced afforded no blue precipitate with sulphat of iron.

Boiled with oxide of mercury carbon was produced, and it would no longer colour iron.

He obtained the same result with oxide of manganese.

Oxide of manganese the same.

Other experiments convinced Mr. Bartholdi, that substances, by which oxigen is abstracted from gallic acid, renders its colour lighter. I rendered, he says, a solution of gallic acid as limpid as distilled water, by boiling it for some time with very pure and well powdered charcoal, of which I took double the weight of the acid; it retained its limpidity as long as I excluded the influence of the atmospheric air from it, and it precipitated iron black.

Disoxigenizing substances whiten gallic acid.

Mr. Bartholdi presumes, that we may thus effect the destruction of its astringent property.

Its astringency thus destructible.

On this I shall not for the present make any observations; as it is necessary to be acquainted with the following experiments, to judge them explicitly.

Extraction of the Gallic Acid.

There are several processes for extracting the acid from galls,

Modes of extracting the gallic acid.

Scheele's process.

On one part of gall-nuts bruised and passed through a coarse sieve, pour six parts of cold water. Let them macerate in a glass jar four days, shaking them frequently: then filter, and expose the liquor to the open air in the same jar, covered only with blotting-paper. In a month's time the liquor will be covered with a thick pellicle of mould, without any precipitate being

Scheele's.

being formed; and it will have lost its astringent taste, but be acid. On leaving the liquor at rest five weeks longer, a precipitate will be formed two fingers thick, and a mumous pellicle above it. The liquor is now to be filtered again, and left anew exposed to the air. At the expiration of some months, the greater part of the liquor will be evaporated: all the precipitates are to be added together, and cold water is to be poured on them; when the liquor has stood to settle, what is clear is to be decanted off: as much hot water as is necessary for the solution is poured on; and by evaporating with a gentle heat yellow crystals will be obtained.

Mr. Bartholdi's process.

Bartholdi's. A tincture of galls in alcohol is to be evaporated; the residuum is to be dissolved in distilled water; and sulphuric acid is to be added to the solution, till the mixture is decidely acid to the taste. In the course of a few hours the extractive matter will fall down, and the supernatant fluid, freed from sulphuric acid by barytes, will yield, according to the author, pure gallic acid.

This process by no means gives this result. It is in general very difficult to seize the moment when all the sulphuric acid is removed by the barytes, since it combines with the gallic acid likewise: and after the liquor is evaporated nothing remains but an acerb matter, containing a great deal of tannin, and insusceptible of crystallization.

Process of Mr. Deyeux.

Deyeux. This chemist discovered, that, by heating bruised nutgalls slowly, and cautiously, in a glass retort, a pretty considerable quantity of lamellated, shining, and silvery crystals was sublimed.

Mr. Richter's process.

Richter's. Nutgalls reduced to a fine powder are to be macerated in cold water, shaking the mixture frequently. After some time the liquor is to be strained off through a cloth: the residuums is to be macerated in a fresh portion of water, and after it has been

been subjected to the press, the two liquors are to be mixed, and evaporated by a very gentle heat. Thus we obtain a dark brown substance, very brittle, which being reduced to a fine powder, and digested in very pure alcohol, tinges it of a very faint straw colour. A second infusion extracts scarcely any colour, and leaves a brown residuum, which is tannin nearly pure. The two alcoholic tinctures are then to be mixed, and distilled in a small retort, till seven-eighths have passed over. The remaining liquor on cooling becomes nearly solid: water is poured on this, and by gently heating a limpid solution is obtained with very little colour.

If this solution be evaporated, very small and very white prismatic crystals are obtained. The mother-water affords yet more, but they are commonly a little coloured: these however, may be rendered very white by washing them with water. By this process a pound of galls affords half an ounce of crystals. They are extremely light.

Gallic acid prepared in the humid way contains tannin.

The processes of Scheele, Deyeux, and Richter, have afforded advantageous results; but they differ with respect to the purity of the acid. The acid produced by the first, as Berthollet observes, retains a great deal of tannin; that by the second is perfectly white; that by the third likewise contains tannin.

In Richter's process too frequent solutions in alcohol decomposes the acid.

By Richter's process the acid, after being purified, is of a pale straw colour. I attempted in vain to bring it to the state of purity mentioned by the author. I found, that if the evaporation, desication, and subsequent solution in alcohol were continued, a certain quantity of acid was decomposed every time; so that the alcoholic tincture, instead of being more transparent, became brown. There is a certain point therefore, where we must stop, if we would preserve the whole of the acid and its properties.

Berthollet purified it by oxide of tin.

Mr. Berthollet tried different modes of purifying Scheele's acid. That which succeeded best with him was treating the acid with oxide of tin recently precipitated from its solution in an acid.

This repeated by the author,

This experiment I repeated. The following is the method I pursued, and the phenomena I observed.

After having separated the oxide of muriat of tin by an alkaline

alkaline base, I washed it well with boiling water, and then boiled it for some time in a fresh quantity of water. I then treated it with gallic acid, and evaporated to the consistence of thick honey. I then added distilled water: and the liquor, after being filtered, was colourless, limpid, without taste, and without smell. On evaporating to dryness nothing remained.

without process.

This difference from the result obtained by Mr. Berthollet led me to suspect, that I had fallen into some error. I therefore repeated the experiment with all the attention possible.

Further trial.

I dissolved 61 grammes of gallic acid, confusedly crystallized and very brown in 500 grammes of boiling water. Part of this solution I set by as a standard of comparison: the rest I boiled with 61 grammes of oxide of tin well washed, and still wet. When about half the liquor was wasted, I made it up to its original weight with fresh water, compared it with the standard, and found it had lost a great deal of its colour. The difference of acidity was scarcely perceptible. It still precipitated glue: but the precipitate was yellow and floculent, while that of the solution not purified was brown, heavy, more copious, and even united in a mass. It appears, that the acid was not yet decomposed: but I could not obtain crystals equally white and pure with those afforded by sublimation, as Mr. Berthollet did.

Did not succeed.

More oxide decomposed the acid.

Desirous of knowing whether a fresh quantity of oxide of tin would deprive the acid of tannin entirely, I added to the liquor 30 grammes of oxide of tin, and evaporated till about 100 grammes of liquor only remained. It passed through the filter clear and colourless, and precipitated neither sulphat of iron nor glue. I could not obtain any gallic acid by evaporation.

This experiment proves, that it is very difficult to free gallic acid completely from tannin; and that by repeating the action of oxide of tin the acid is decomposed. Thus no doubt Mr. Proust proceeded; for this chemist observed, in his memoir printed in the *Annales de Chimie*, vol. 42., that the oxide of tin he employed to purify the gallic acid afforded him as a product only a colourless insipid liquor, without taste, and not having the slightest effect on solutions of iron or tincture of litmus.

Proust found the same,

Bartholdi's experiments repeated.

As to the means proposed by Mr. Bartholdi, I do not imagine they can be employed. Yet, as the author neglected to examine the products of his operations, I thought it necessary

to

to repeat his experiments, and determine the nature of the results that might arise from them. With this view I poured a solution of gallic acid or red oxide of mercury; which immediately became brown, and gradually changed to black. The solution too acquired a deep brown tint. In this state it was still acid, gave a blue colour to a solution of sulphat of iron, and precipitated glue; but it contained no mercury.

With red oxide of mercury.

I boiled this liquor on a fresh quantity of oxide; when it became clear, colourless, and no longer contained either tannin or gallic acid.

Part of the oxide of mercury was reduced: the rest was mixed with concrete phosphoric acid [so the original], but nothing was sublimed from it by the action of lead.

If charcoal previously purified be employed instead of red oxide of mercury, the solution of gallic acid loses almost entirely its taste and colour; the liquor becomes green, and no longer precipitates glue; but it still gives a violet blue tint to solution of sulphat of iron. Boiled with a fresh quantity of charcoal, the liquid becomes colourless, and no longer produces, any change in the solution of glue or of sulphat of iron. After it is evaporated to dryness, a brown matter remains in the capsule, which precipitates acetat of lead of a dirty gray, and nitrat of mercury and muriat of tin yellow; so that we may consider it as extractive matter.

With charcoal.

These experiments prove, that there exists no process for purifying Scheele's gallic acid but sublimation; unless the proportion of oxide of tin employed by Mr. Berthollet, which he does not mention, has a great influence on the result. Yet the mode of purifying the gallic acid by sublimation cannot be adopted, if we wish it to retain all its properties. The different characters exhibited by the two acids will afford proofs of this assertion.

Only to be purified by sublimation,

but this alters its properties.

Comparison of the crystallized and sublimed Gallic Acids.

Scheele's crystallized acid imparts to water a slight lemon colour: this solution grows deeper coloured by the action of the air: it reddens tincture of litmus: limewater produces in it a blue colour, which changes to that of peach blossoms if the limewater be in excess, and on adding a few drops of nitric acid to a rose colour. The same phenomena take place with water of barytes.

Solution of the crystallized acid,

with lime water,

barytes,

This

alkalies, This solution takes a colour more or less green with carbonat of soda, but is not changed by carbonat of ammonia. Caustic potash changes it to a deep brown; and ammonia to a reddish brown.

sulphat of iron, nitrat of mercury, acetat of lead, muriat of tin, With green sulphat of iron it is a violet blue, which is constant, as an excess does not alter it. With nitrat of mercury it gives a yellow precipitate; with acetat of lead, and muriat of tin, a white.

oxigenized muriatic acid, The solution of this acid is not altered in appearance by oxigenized muriatic acid.

and glue. With glue it gives a copious precipitate.

Richter's acid. The same experiments were made with the acid obtained by Richter's process, and the results were similar, except that the precipitate thrown down by glue was very abundant.

Solution of sublimed acid, *Sublimed acid of Deyeux.* The solution of this acid by hot water emits an aromatic odour, and a slight oily pellicle is perceptible on its surface.

with lime-water, barytes, This solution becomes brown by exposure to the air. It faintly reddens tincture of litmus: limewater gives it a colour of wine lees, which an excess of it converts to a fawn colour. With barytes we obtain the latter tint, and the liquor is immediately covered with an oily pellicle.

carbonated and pure alkalies, Carbonat of ammonia produces no change in the acid liquor: that of soda gives it a fawn colour. Caustic potash browns it considerably: with ammonia the colour is lighter.

sulphat of iron, If a few drops of a solution of sulphat of iron be dropped into this acid liquor, a blue colour is produced, which soon changes to a violet blue. Frequently however, instead of a blue colour, we have a deep green. This no doubt depends on some peculiar circumstances: and I conceive it may be attributed to the degree of oxidation of the iron; for with muriat of iron at a maximum we have constantly a green colour. This effect is less striking with other acids: the infusion of galls, made without lead, always retains its pure blue colour.

With nitrat of mercury the precipitate is blackish: that with acetat of lead is fawn coloured, and very light.

nitrat of mercury, acetat of lead, other metallic salts, The sulphats of zinc and copper, and muriat of tin, produce no change.

Oxigen-

Oxigenized muriatic acid browns the solution of gallic acid, and an excess deprives it of colour. and oxigenized muriatic acid.

On comparing the difference of the effects of these acids, it will be easy to appreciate them.

The sublimed acid has less acidity: it is decomposed by the air: it has no action on barytes, carbonat of ammonia, or muriat of tin. The precipitate obtained with nitrat of murcury is blackish, instead of yellow; that with acetat of lead is slight and fawn coloured, instead of copious and white. The sublimed acid compared with the crystallized.

Oxigenized muriatic acid browns a transparent and colourless solution of the sublimed acid, while it does not alter the colour of a solution of the crystallized.

Lastly the sublimed acid does not constantly produce the same colour with sulphat of iron, and does not precipitate glue.

If it be easy to point out the characters that distingu sh these two acids, it is difficult to explain whence their difference arises. Mr. Berthollet has justly observed, that Scheele's acid, when not purified, contains a great deal of tannin; and that, when purified by oxide of tin, it does not percipitate glue. Not easy to account for the differences.

As to that of Mr. Richter, I have already pointed out its analogy to Scheelés: yet both these acids appear to me to differ from that obtained by sublimation. The latter contains a small quantity of volatile oil, which is combined with it; and which by the action of caloric assumes a character approaching to that of oils rendered resinous. This property may be ascertained by dissolving the sublimed acid either in alcohol, or in ether; for if the liquid be evaporated by rubbing it on the skin, we shall experience an effect similar to that produced by a resin dissolved in alcohol. Sublimed acid contains a volatile oil.

It is not without difficulty, as may be supposed, that we can attain a complete knowledge of the nature of the gallic acid. Does this acid exist in galls already formed? May we consider it as a peculiar acid; or rather is it merely the result of the combination of a vegetable acid with tannin, extractive matter, and other substances existing in galls? These are questions, that yet remains to be solved. I have attempted by a series of experiments to add some facts to those that are known; and if they do not yet lead to a complete solution I conceive some new results will be found in them, which serve to explain the nature and properties of the gallic acid. What is the gallic acid?

Examination of the Action of Caloric and of Water on Nutgalls.

Action of Caloric.

Action of caloric.

Mr. Deyeux having examined in a particular manner all the products of the distillation of galls on a naked fire, I shall consider only the acid liquor obtained from them.

Properties of the distilled fluid.

The process was conducted in the manner indicated by that chemist. The fluid in the receiver was aromatic, a little milky, very acid, did not precipitate glue, and gave a violet blue with sulphat of iron, which changed to a dirty green. Lime and barytes produced a peach-blossom colour. Nitrat of mercury threw down a blackish prcipitate; acetat of lead, and muriat of tin, a white.

Saturated with potash, gave signs of acetic acid.

Having saturated the acid liquor with potash, I obtained by evaporation a brown empyreumatic matter, which on the addition of sulphuric acid emitted a pungent smell resembling that of acetic acid.

Action of Water on Nutgalls.

Gall macerated in water.

Galls finely powdered being shaken in cold water for four minutes, the liquor, when filtered, was of a golden yellow colour. One part was distilled in a retort on a sand heat: the other was saturated with carbonat of soda.

Distilled gave an acidulous fluid.

The produce of the distillation was a clear, colourless, and slightly acid liquor, that precipitated neither glue nor sulphat of iron.

Saturated with an alkali, sulphuric acid added and distilled.

The liquor saturated with the alkali was evaporated to dryness; and the residuum being dissolved in distilled water, sulphuric acid was added till it was a little in excess, when the mixture was distilled in a retort. The products were examined in succession. First a fluid came over without taste or smell: soon after the liquor was acid, but contained neither sulphuric nor gallic acid.

With boiling water.

I made a similar experiment with boiling instead of cold water. The liquor remained turbid, though filtered. Being subjected to distillation, and combined with soda, in the same manner as the preceding, I obtained the same results.

These

These experiments suggested to me the existence of a acid ready formed in galls, and the possibility of obtaining it by distillation. A free acid in galls.

Accordingly I heated to ebullition in a common alembic a kilagramme [2lb. 3oz. 5dr. avoird.] of galls coarsely powdered, with double the weight of water. The distilled liquor, as Mr. Deyeux observed, was a little milky, aromatic, and on standing deposited a little floculent sediment. I changed the receiver, when about two thirds of the liquor had come over, and I continued the distillation till it became coloured. Obtained by distilling with water.

The first product was acid; reddened tincture of litmus; and had no action on lime or barytes water, nitrat of mercury, acetat of lead, sulphat of iron, or glue. 1st. product.

The second product was turbid, coloured, a little empyreumatic; its acidity was more marked; and it precipitated the metallic solutions above mentioned, but did not act on glue. 2nd. product.

Each of these acid liquors was saturated with potash. The first yielded a foliated salt, which, on the addition of sulphuric acid, gave out a smell of asetic acid. Part of this salt was dissolved in distilled water: the excess of its base was accurately saturated by nitric acid, and nitrat of mercury at a minimum was added to the solution; when a precipitate was formed, which had all the characters of acetat of mercury. To convince myself still farther of the presence of acetic acid, I treated the neutral acetat of potash in the same manner, and it afforded me the same results. Saturated with potash. Proofs of the acid being the acetic.

The second product was saturated with potash in the same manner. The liquor became very brown: a slight pellicle formed on the surface, which increased during the evaporation: the saline matter was highly coloured and empyreumatic. Being subjected to the same trials as the preceding, similar appearances were observed. 2nd. product afforded similar proofs.

These experiments leave no doubt of the presence of acetic acid in galls: they prove, that it may be obtained by distillation with water; and that caloric, when it acts more directly on this acid, facilitates its combination with a small quantity of empyreumatic oil, and perhaps with a little tannin, the presence of which is not demonstrable by glue: but as this liquor acts on sulphat of iron in the same manner as the sub- The acetic acid. by means of caloric is combined with an empyreumatic oil, and perhaps tannin.

and with aromatic oil, instead of empyreumatic, when sublimed.

lined acid, we must presume, that there is a kind of analogy in their composition; admitting however this difference, that the sublimed acid contains no empyreumatic oil, but a particular aromatic volatile oil.

The oil shown.

This oil may be detected by dissolving the acid in very pure sulphuric ether, and adding a little water, when a few drops of oil will be seen floating on the surface, which disappear on shaking the mixture.

Caustic potash.

If a concentrated solution of caustic potash be employed instead of water, a white, milky substance is separated, which requires a large proportion of water to dissolve it, but the liquor still remains turbid.

The ethereal tincture,

This ethereal tincture yields a fine blue colour with sulphat of iron.

and its residuum.

Evaporated in the open air it leaves a shining substance, very acid, separating in scales, and having the appearance of a varnish.

The same phenomena take place, if galls be digested in ether; but the substance contains tannin in addition.

Examination of some earthy and alkaline gallats.

Farther proofs of acetic acid

Though it appears to be demonstrated, that acetic acid exists ready formed in galls we cannot too much multiply proofs in confirmation of its presence; and to demonstrate, that this acid, combined with other substances, constitutes the gallic acid.

by forming different gallats.

With this view I formed gallats of lime, barytes, potash, and soda. These neutral combinations afforded a violet red colour with a solution of sulphat of iron, and scarcely precipitated glue while the acid employed had the property of forming with it a copious precipitate. On these salts dried I poured some very weak sulphuric acid; I distilled them with a gentle heat, and I always obtained acetic acid.

The residuum afford no proof of a gallic acid.

The retorts contained a very deep brown matter. I crystallized the salts that were preceptible of it, and obtained sulphats.

The supernatant mother-water had the property of slightly browning the solution of sulphat of iron; but this appearance does not prove the presence of gallic acid, for the black colour of the mother-water was sufficient to give it this hue.

If

If one of the gallats, that of soda for instance, be treated with charcoal, the tannin will be entirely destroyed, so that the solution will no longer precipitate glue; and after repeated boiling with fresh portions of charcoal, it will no longer act on sulphat of iron. Gallats treated with charcoal,

The liquor being afterward evaporated to dryness, and distilled with very weak sulphuric acid, we still obtain acetic acid. still give acetic acid.

I shall not insist any farther on the possibility of obtaining acetic acid by decomposing gallic acid. I might mention the experiments, which would tend to support the preceding, but entering too minutely into these particulars would add nothing to the facts I have already adduced.

I shall conclude with an experiment, which appears to me important. The object was, to establish the nature of the elastic fluids resulting from the complete decomposition of the gallic acid by heat. Mr. Deyeux has announced, that he obtained only oxigen gas, and carbon. Mr. Berthollet, who repeated the experiment, says, that he had no oxigen gas, but constantly carbonic acid. Examination of the gasses produced by decomposing gallic acid.

These results, of which no other vegetable acid furnishes an example, could not avoid exciting the attention of chemists. In fact it is difficult not to admit hidrogen in the composition of gallic acid; and Mr. Fourcroy has expressed his doubts on this subject in his System of Chemical Knowledge, but the question is not yet decided by experiment.

In consequence I heated gallic acid in a retort. The fire was gradually raised till the retort was red hot. During this action of caloric I obtained several jars full of elastic fluid. The first contained only atmospheric air; the others carbonic acid gas: at least the gas had all the characters of thin acid; but the phenomena that occurred during the decomposition of the gallic acid led me to suspect, that, if any hidrogen gas had been evolved, it could exist only in a very small quantity. I did not satisfy myself therefore with the trial by limewater, and the extinction of a taper in the gas. Having perceived, that hidrogen gas mixed with a great deal of carbonic acid gas cannot be fried, because this acid acts too promptly on the flame of the taper, I passed a little caustic potash into the last jar of gas; agitated it, absorb the carbonic acid; and then immersed a taper in the residual gas, which burnt with flame, and thus afforded me a proof of the presence of carburetted hidrogen. The first portion the air in the retort, afterward carbonic acid gas, mixed with carburetted hidrogen.

The

Base of gallic acid, hidrogen and carbon.

The gallic then, like other vegetable acids, is composed of oxigen, hidrogen, and carbon. If but a small quantity of hidrogen, can be obtained, it is because water is formed during the decomposition of the acid, so that the hidrogen passes over only when very little oxigen remains to act on the carbon.

I have attempted to shew, that the gallic acid is a compound. Its formation by Scheele's process appears to me to favour this opinion. In fact, if the quantity of acid extracted from the aqulous infusion exposed to the air be compared with that afforded by sublimation, I conceive it is not difficult to account for the increase. There can be no doubt, that acetic acid

Acid of Scheele.

med in the liquor, which, acting on a portion of tannin and extractive matter, constitute the gallic acid of Scheele: but this combination is rendered more intimate, and somewhat different, by the action of caloric; of which we have a proof when the acid is obtained by sublimation, for not only is the tannin decomposed, but the acid remains combined with a volatile oil which is formed. Perhaps this acid contains a small portion of tannin in very intimate combination, whence no doubt arises its property of giving a momentary blue with sulphat of iron, though its presence cannot be demonstrated. This acid then must have different properties from that of Scheele: and if it were possible to assimilate it to other vegetable acids, the benzoric would it be that, which it would have the greatest analogy. May it be considered however as a modification of the gallic acid? I think not. It is the same with other vegetable acids: and it is probable, that there exists no modification of them. The acetous appears to be the sole vegetable acid: it desolves and retains in various proportions a number of the immediate products of vegetables, and in the processes to which we subject vegetable substances, we facilitate its combination in a more intimate manner; and frequently even augment the quantity of this acid. Already several chemists have admitted the possibility of the acetic acid's dissolving and remaining combined with fixed and empyreumatic oils, and animal matters: they have even gone so far as to imitate acids of this sort. The formic, pyrolipic, pyrotartarous, and pyromucous, have been classed by Messrs. Fourcroy and Vauquelin among the compound acids: it is the same with the

Sublimed acid,

not a modification of the gallic.

Acetous the only vegetable acid, forming others by various additions intimately combined with it.

Instances.

lactic,

lactic, the composition of which was pointed out at the same time by those chemists, Mr. Thenard, and myself: lastly we have proof too, according to Mr. Thenard, of the existance of this acid in the urine and sweat, as well as in the sebacic and ozonic acids. I might farther add to these observations (if we were not persuaded, that the acetic acid is found every where) that it exists in the vegetable as in animal matters, where it is almost always in a state of combination; and that, an equilibrium in the proportions being once established, it gives rise to compounds hitherto unalterable, and the affinity of which cannot be destroyed, but by reducing them to their primary elements, oxigen, hidrogen, carbon, and nitrogen.

From the facts announced in this memoir it follows:

Recapitulation,

1st, That the gallic acids of Scheele and of Richter differ essentially from that obtained by sublimation; and that the crystallized is preferable as a reagent, on account of the constant uniformity of the colour it gives with iron.

2ndly, That this acid appears to be composed of acetic acid, tannin, and extractive matter; and that it cannot be completely freed from tannin by crystallization.

3dly, That the acid obtained by sublimation contains no tannin, at least that can be ascertained by acting on glue; and that it cannot, on any occasion, supply the place of the crystallized acid.

4thly, That the sublimed acid, appears likewise to be composed of acetic acid, united with a peculiar aromatic volatile oil.

5thly, That by means of water, poured into the ethereal tincture of galls, or ether containing the sublimed acid, an oily matter is separated.

6thly, That there is no process known for purifying Scheele's acid completely: that is to say, we cannot take from it the whole of its tannin, without reducing it to the state of acetic acid; which proves, that the portion of tannin it retains is necessary to constitute gallic acid, and that to this are owing its excellent properties in the art of dying.

7thly, That the red oxide of mercury, and oxide of tin, as well as carbon, decompose this acid.

8thly, That by distilling galls with water acetic acid may be obtained;

obtained; and that it is by the assistance of caloric acting more immediately on galls, that a more intimate union between the acid and the tannin is effected.

9thly, That the earthy and alkaline gallats likewise afford acetic acid by their decomposition.

10thly, and finally, That gallic acid, like the other vegetable acids, is composed of oxigen, hidrogen, and carbon.

If these results be accurate, we may conceive it possible, to accomplish its synthesis; or some trials that I have already made give me the hope of succeeding in it. I shall do myself the honour of imparting the farther results of my inquiry to the class, if they should be worthy its notice.

XI.

Observations on the Soda, Magnesia, and Lime, contained in the Water of the Ocean; shewing that they operate advantageously there by neutralizing Acids, and among others the Septic Acid, and that Sea-Water may be rendered fit for washing Clothes without the Aid of Soap. By SAMUEL L. MITCHILL, *of New York.*

(*Continued from p.* 392 of Vol. XVI.)

Observations and facts respecting the component parts contained in sea water, and the useful application of that fluid.

I FIND on experiment that carbonate of soda thrown into ocean water, immediately renders it turbid, the lime and magnesia instantly turning milky on their disengagement from their respective portions of acid. To make the water fit for washing, so much soda must be added as not only to effect a complete precipitation of these earths, but to render the water sufficiently lixivial or alkaline. It will then exert its detergent and purifying powers.

Having entertained doubts at first, whether the water ought not to be decanted of after the lime and magnesia had settled to the bottom, or whether it would not require straining or filtering to render it fit for use, I convinced myself by experiment

ment that foul linen could be rendered clean and white by being washed in alkalized ocean water which contained its whole quantity of precipitated earth diffused through it. I rather think the small quantity of those impalpable and white particles which adhere to the linen worn upon the body will be advantageous and wholesome, as the shirts and other garments will thereby be enabled to neutralize a portion of the acid and oftentimes noxious matter formed from the sweat and other excretions of the skin, &c. Thus they will be rather serviceable than otherwise, and as both are in their carbonated state (having borrowed fixed air from the soda) they cannot do any harm.

Observations and facts respecting the component parts contained in sea water, and the useful applications of that fluid.

The general inferences from the whole of the preceding reasoning are these: 1. Alkaline substances, such as magnesia and more powerfully lime and soda, are plentifully distributed through the ocean, to keep it from becoming foul, unhealthy and uninhabitable, which doubtless would be the case if the sulphuric, septic, and muriatic acids abounding in it were not neutralized. 2. Where either of these acids is but imperfectly saturated, as happens when they are united with magnesia and lime, they decompound soap, let loose its grease, and become unfit for washing by aid of that material. 3. If soda or barilla is added to ocean water in sufficient quantity and the water lixiviated or alkalized the earths will of course be precipitated and the acids neutralized. 4. In this state, dirty linen may be cleansed in it; and men at sea be thus enabled to have their clothes washed without the aid either of soap or of *fresh* water. 5. For this purpose, a quantity of barilla or soda should always be provided as an article of the ship's stores, and issued to the men on washing days. 6. Thus by the operation of this alkaline salt, a great proportion of the nastiness and infection bred in the clothes, bedding and berths of persons at sea might be prevented, and the crews and passengers so far preserved from fevers and dysenterses. 7. No more room would be occupied by water casks in the holds of vessels, than at present. 8. The small quantity of magnesia and lime adhering to clothes washed in this way, is an advantage over and above what takes place in using fresh water. And 9. A broad and noble view is opened of the economy of Providence in distributing alkaline salts and earths, so liberally throughout the terraqueous globe.

XII.

An Account of the Improvement of an extensive Tract of Land. By* Richard Phillips, *Esq.*

SIR,

Improvement of waste lands.

In the year 1804 the waste lands in the township of Bron-y-garth, in the parish of St. Martin in Shropshire, were divided and allotted by an agreement entered into by the proprietors of land, without any application to Parliament.

This township is separated from the county of Denbigh by Offa's Dyke, the boundary in ancient times between the kingdoms of Mercia and Wales: the boundary here, as in other uncultivated parts of the demarkation, still remains entire, after a lapse of 1000 years. Upon the ground, where the improvements detailed in my paper to you are made, the descendants of the ancient Britons fought for their independence, and for what remained of their territories. Upon this spot the bands of Henry II. headed by that monarch himself, were foiled in the battle of Ceiriog by Owen Gwenydd, at the head of his brave Welshmen. The township on the west of Offa's Dyke, is called *Crogen*, i. e. *a place of graves*, because there the slain, who had fallen in battle, were buried. The posterity of the two, once hostile nations, now contend which shall excel most in the arts of peace. This rude soil is now no longer fertallized by the blood of warriors, but by the united labours of Englishmen and Welshmen. The dyke is still pretty accurately the line which separates the two langnages: Welsh is generally spoken on the western side; English on the eastern. The hills, of which these wastes form a part, are at least as high as any in the county. Mr. Archdeacon Corbet, in his account of the agriculture of the county of Salop, asserts, that the hills near Oswestry are the highest in Shropshire.

The lands in question are part of the same chain which composes the skirts of the Berwyn, a mountainous tract, extending widely over the west of Denbighshire, and the contiguous part of Merionethshire. As a traveller ap-

* Society of Arts, 1796.

proaches

proaches this country from Shrewsbury, a line of highly elevated ground presents itself to his view, extending from near the Severn to the neighbourhood of Wrexham. This ground once formed the rampart of Wales, though now cultivation in several of its parts is softening the roughness of its aspect. Improvement of waste lands.

The continuity of the line of hills is broken by two principal valleys; the larger is that of Llangollen, through which the Dee flows: the other is watered by the stream of the Ceiriog. One part of the Bron-y-garth enclosure looks over the last-mentioned valley, and has a northern aspect; the other looks to the east, over the plains of Shropshire.

Lime is found in every part of the line which divides the mountains from the plains, on the frontier of North Wales. The beds of lime-stone in some places lie on sand-stone, and in other places are found below it. In others again the lime-stone is near the bottom of a hill, sand-stone occupies the middle space, and lime-stone is again found upon the summit.

In some respects the sand-stone works well, and is of a superior quality, as the aqueducts over the Dee at Ponteysyllty, and that over the Ceiriog at Chirk, sufficiently prove. The sand-stone in the quarries, which furnished materials for building these aqueducts, is perhaps equal in beauty and durability to Bath or Portland stone; and the lime-stone, at least in one quarry near Oswestry, becomes a beautiful black marble. In the lands, spoken of below, the lime-stone supplied me with manure, and the sand-stone forms the larger portion of my fences.

The paper, which accompanies this letter, is drawn up in haste, because it was only very lately that I determined to be a candidate for the notice of your honourable Society. But all the parts are faithfully and accurately stated.

I am, Sir,

Your most obedient Servant,

RICHARD PHILLIPS.

Tyn-y-Rhos, near Oswestry,
January 1806.

To C. Taylor, M. D.

An

Improvement of waste lands.

An Account of the Improvement of more than Ninety Acres of Land lying waste.

In the year 1804 a large quantity of waste land was divided and allotted in the township where I live, on the borders of North Wales, by private agreement. I became possessed, as proprietor, of seventy acres of these lands. I obtained fifty acres more by two leases, each for twenty one years.

The wastes consisted of two divisions. The first was a piece of common land, surrounded by old enclosures. This portion, though raised far above the general level of the country, is much less elevated than the larger tract hereafter to be described.

The portion of this waste allotted to me was eight acres. The grass produced, while the land was in its natural state, was a sour rough sort. It afforded pasture in the summer to a few cattle, horses, and sheep. The coldness of the soil, and the consequent bad quality of the grass, gave this common the Welsh name of *Rhos*, a name which implies a tract of moist land, producing a coarse sour herbage.

1. I began my improvements upon this allotment, because it lay near my house. The fence is a bank four feet high from the bottom of the ditch, with a double rail at the top. A double row of quick is planted upon the top of the fence, to supply the place of the rails when they decay.

The surface soil is about six inches deep, with a substratum of bad yellow clay. The first ploughing was in June 1804. It was cross-ploughed and harrowed in August; ploughed a third time about the 20th of September; manured about the end of the same month with one thousand six hundred and ninety bushels of lime, amounting to about two hundred and eleven bushels an acre; ploughed a fourth time in the middle of October, in small butts or ridges; sown and harrowed. This operation of ridging was peculiarly necessary here to carry off the surface water, which had formerly greatly injured the land. Twenty-four bushels of Devonshire wheat were sown: the return was about two hundred and forty bushels (thirty bushels an acre). The crop was one of the finest in the county. The expences, as appear by the subjoined table, were

£. s. s.

£. 88 19*s*. 1*d*. The wheat was worth last month £. 130. The balance in my favour is £. 40 0*s*. 11*d*. (£.5 2*s*. 7*d*. an acre). This land in its natural state was not worth five shillings an acre. When it is laid down in grass it will be worth 40*s*. an acre. Improvement of waste lands.

In the beginning of October 1805, the stubble was harrowed off, and conveyed to the farm-yard. The land was then ploughed, sowed with twenty-four bushels of wheat, and harrowed as the year before. This is not my usual course of crops; but it was thought that old common land could not very easily be exhausted, and I was tempted to take another crop of wheat by the high price of corn, and by the circumstance of the land being for four years tithe free. The corn now, the 12th of January, is coming up in abundance.

It is my intention to lay down this lot with grass seeds, to be sown with oats in the spring of 1807. Oats I conceive to be the best grain for the next crop, because the land is not dry enough for turnips and barley.

The second, and much larger, division of lands lying waste extended along the side, and reaches the summit of a hill, which is equal in height to any in this county. The aspect is, for the most part, north and north-east. A mountain torrent runs through the midst of this tract: some of the lands on one side of this torrent are more sheltered, and have a southern aspect.

Lime-stone is found on the lowest part of this waste, not far from the bed of a river: but the steepness of the ground above would have been too formidable an obstacle to the cultivation of the higher lands, had not lime-stone been discovered upon a spot so elevated, as to enable the improver to convey his manure, at a comparatively light expence, to the lands below.

The coals indeed, for burning the lime, are brought up a steep hill, a distance of four miles. The ascent up which they are conveyed, enhances considerably the expence of the manure.

Upon this waste the lime-stone is at the bottom of the hill, and fortunately upon the top also. The substratum, at no great distance from the surface, is sand-stone, in some places

Improvement of waste lands.

places hard, in others loose, and less useful for fencing; a purpose to which I have applied it in dividing most of the enclosures.

All the waste lands allotted to me as proprietor, or occupied by me as tenant, in consequence of the two leases mentioned above, were covered for the most part with *gorse*, (Ulex Europæus,) in some parts of England called *furze*. Some more favoured spots produced fern alone; and others were much encumbered with stones. The stones were carted off the lands to assist in making the fences; and those, which were too small for this purpose, were used to fill up large holes in various parts of the land.

The thin soils upon these wastes seems to have been created by the annual decay of portions of the gorse; a plant admirably calculated to produce, and afterwards to detain, in spite of rains and storms, the vegetable earth upon these steep declivities. Around each bush of gorse is always found a heap, more or less high, of excellent soil; and so completely do the prickles of this plant defend the grasses, that grow among it, from the attacks of sheep, that the earth, produced by the successive decay of vegetable matter, accumulates, and renders lands, which a few centuries ago would probably have been unproductive, proper for the growth of corn.

It is impossible to traverse our mountains without observing how wisely these things are contrived by Him who provides for us all.

The highest mountains of North Wales, where the rock does not every where appear, are clothed with heath. As ages roll by, the soil, produced by the annual decay of portions of the heath, becomes fit to produce gorse. If the water has a ready fall, and the land is dry, gorse appears in abundance on the more exposed sides of the mountains. Where soil has accumulated in sufficient quantities, the next protector and fertilizer of the mountains is fern. Wherever this plant flourishes, still richer quantities of vegetable earth are every year added to the surface soil, and the ground is rapidly prepared for the plough.

Let me be excused for having made this digression longer than I intended.

I now

I now proceed to state the operations performed upon the second portion of waste land improved by me. Improvement of waste lands.

2. One close of 21 acres, for which I have a lease for 21 years, at 10*s.* an acre, is so steep, that no waggon or cart can be used, to carry off the crop; drags must be employed for this purpose.

This land was so steep, and was incumbered with such a quantity of stones, that a respectable gentleman farmer, whose lands are contiguous to it, and to whom it was offered in exchange for other lands, declared he would not cultivate it if it were given him as a present. I should observe, that it was stipulated in my lease, that the landlord was to be at the whole expense of fencing.

The greater part of this land was begun to be ploughed for me in December 1804 by a neighbouring farmer at 20*s.* per acre. It was at first ploughed one way. The steepness of the ground made it necessary for the horses to drag the unencumbered plough to begin the furrow again upon the "vantage ground." Two acres of it could not at first be ploughed at all. Hand labour was here employed.

The difficulty of ploughing proved so great, that I thought it right to make some addition to the stipulated price of 20*s.* an acre. It was harrowed in June 1805. The whole of the field was cross ploughed in July; harrowed and manured in August with 5200 bushels of lime, about 250 bushels an acre. The quantity of lime generally used in this country is about one-fourth less than this. The lime was carted in small quantities, and laid upon the land with the assistance of three men with each team. So many men were necessary on account of the unevenness of the ground.

The fence, made at my landlord's expence, consists of a wall six feet high, 20 inches broad at the base, and 14 at the top. It is to be pointed next summer with mortar. The materials were partly stones collected in the field and partly sand-stone obtained from a quarry, opened for this purpose in an allotment to be described hereafter. The fencing is mentioned in this place, because it was in this part of the process that the stones were collected off the land. The land

was

Improvement of waste lands.

was ploughed a third time the first week in October; sowed with wheat, and harrowed. Three ploughings were thought sufficient for this land, because the soil is light and ragged. The depth of soil is here near eight inches. The substratum is a light yellow *rammel*, called in this country *cat-brain*. The sand-stone, which lies next below, does not appear near the surface, except in one small part of this field. The wheat sown was 71 bushels, above three bushels an acre. This large quantity of seed was thought necessary, on account of the lightness of the soil, and the exposed northern aspect. These plants now (January 12) look healthy; they are of a good colour, and equal in appearance and promise of a good crop to the wheats upon the best lands in the neighbourhood.

3. I obtained a like lease for 21 years of another lot of 12 acres from the same landlord at 10*s*. an acre. I may here remark, that by an error in laying out a road, nine and a half acres of this field belong to my landlord, the other part to me as proprietor. But the close is at present undivided, and the whole subject to the same management. The aspect is here S. E.; but the situation is much higher than that of the last mentioned lot.

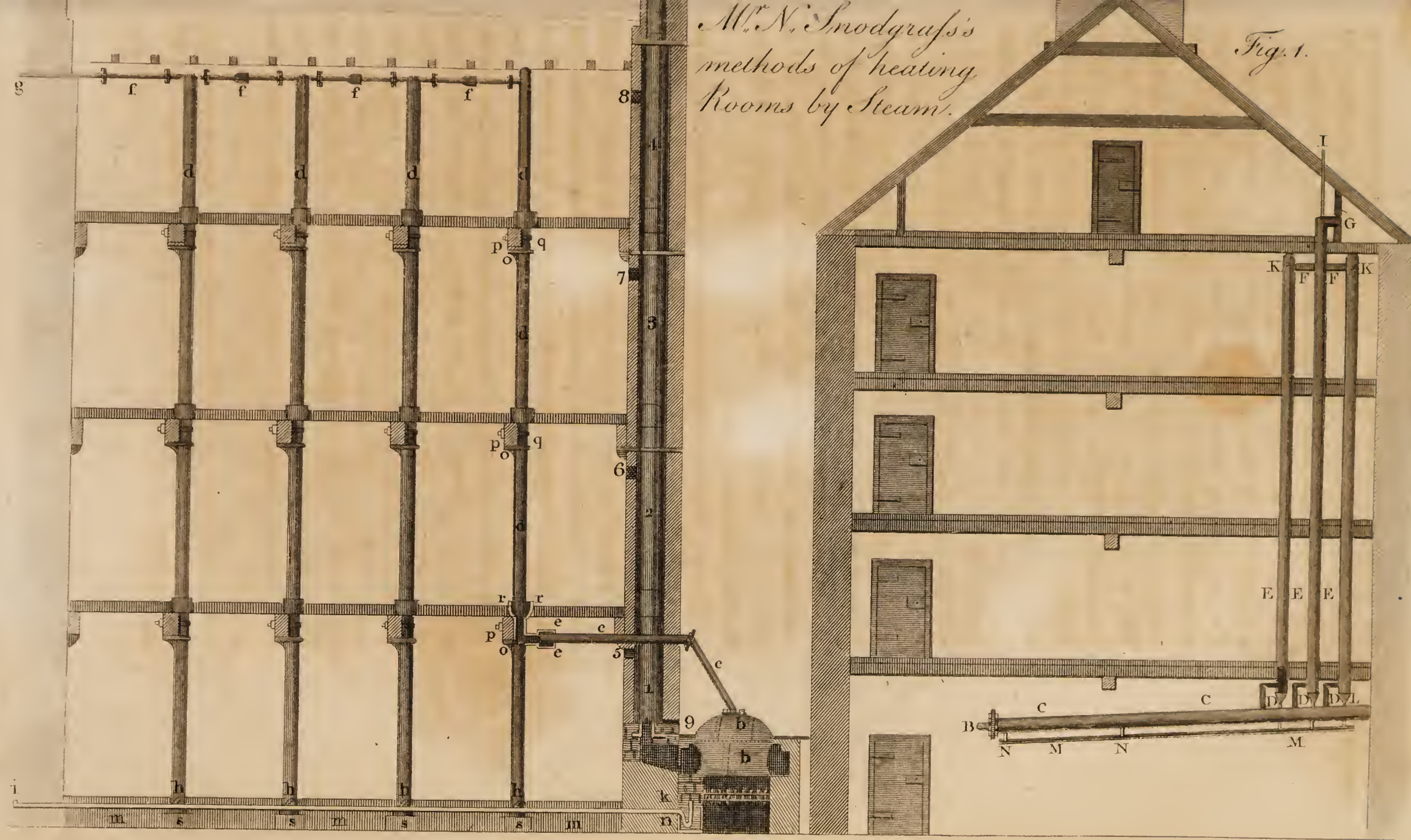
This lot, like the last, was, by the terms of my lease, to be fenced by the landlord; but all the fences have been made under my superintendance. The whole fence would have been a wall, but the sand-stone rock on this part of the hill failed. Twenty-six roods are fenced with a stone wall, six feet high. Sixty-seven roods are fenced by a bank and ditch, faced on the one side with stone, and protected above by posts and double rails. Upon the top of the bank hawthorn quick is set. Fifty-eight roods more, which complete the enclosure, are bounded by a very high old dyke. This boundary, however, is of such a sloping form, that some additional defence was necessary. A ditch is therefore sunk on the summit of the dyke, to the depth of five feet; in this are planted strong *staggers*, as they are here called, consisting of hazle, holly, thorns, and horse briers.*

* So called, perhaps, because they are an excellent fence, when mixed with other underwood, against horses as well as sheep, and other animals.

The

Mr. N. Snodgrass's methods of heating Rooms by Steam.

Fig. 1.



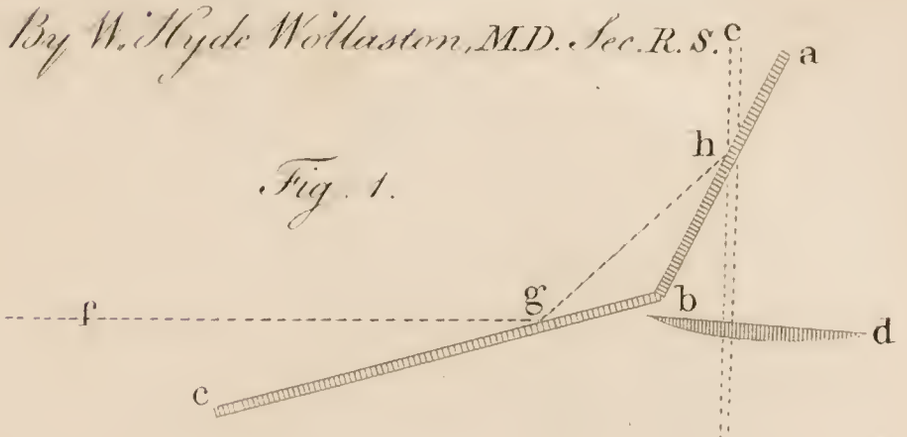
Nicholson's Philos. Journal, Vol. XVII. pl. 2. p. 80.

Mr. Brewster's Astrometer.

Camera lucida.

By W. Hyde Wollaston, M.D. Sec. R.S.

Fig. 1.



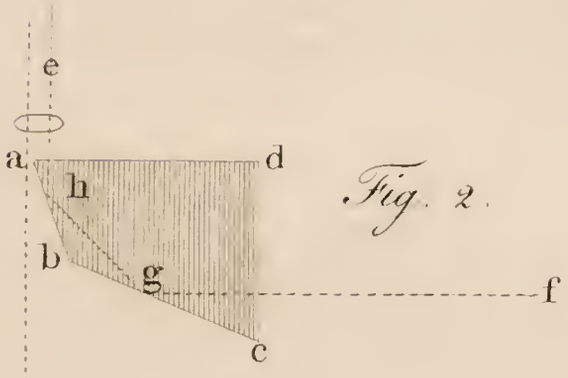
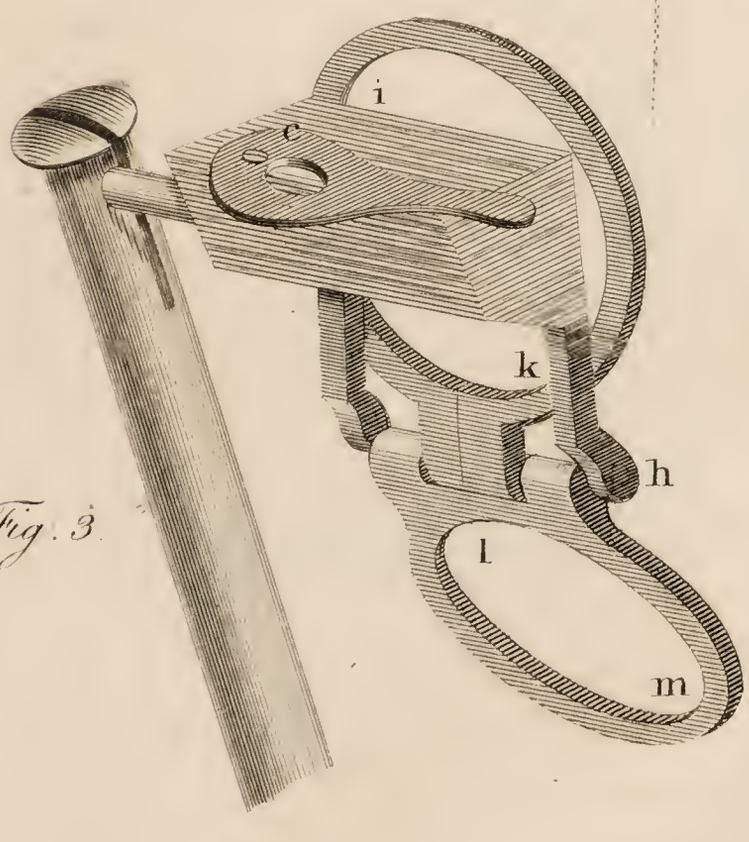


Fig. 2.

Fig. 3.



Decomposition of Light.

Fig. 1.

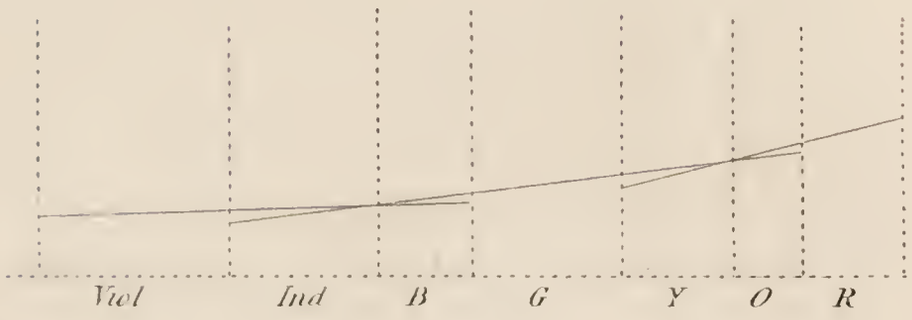
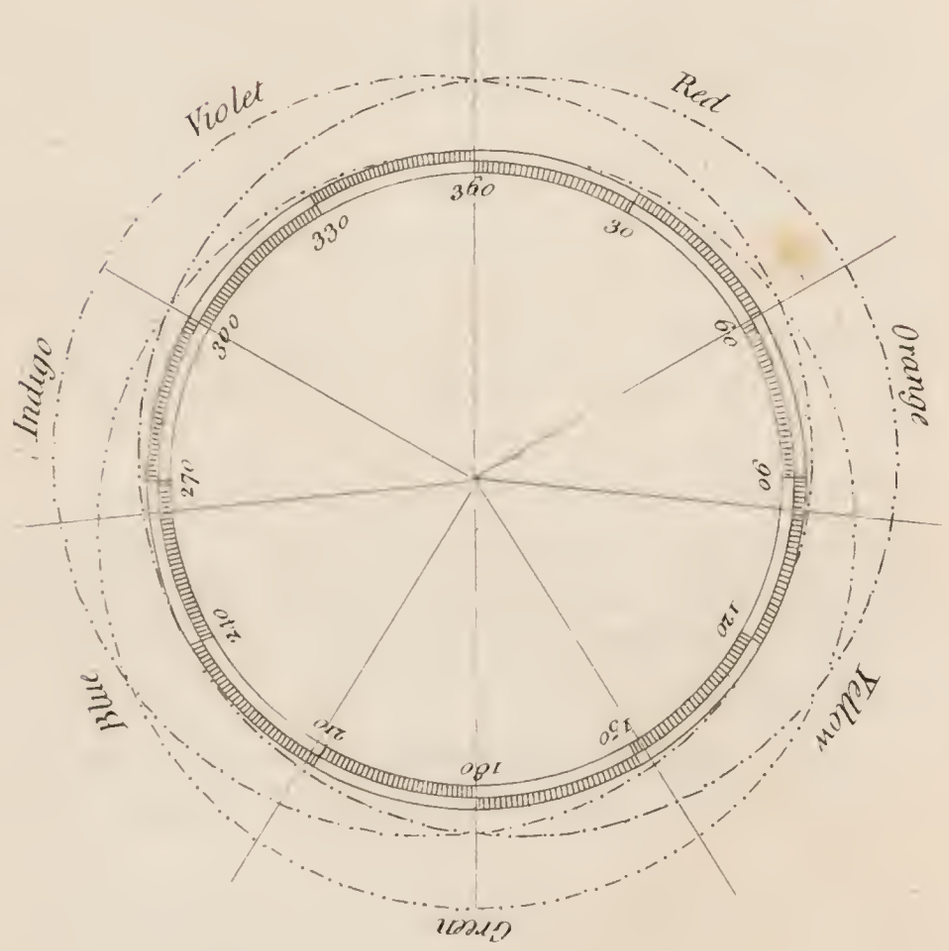


Fig. 2.



Improvement of waste lands.

The first ploughing was in February 1805: it was done by my own teams. We used four horses in each team. I will remark here, by the way, that my own teams ploughed, the first time over, all the lands which I have improved, except the last mentioned lot of 21 acres; and that the expence for oats only given to my horses the first nine months of my improvements was 70*l*. This lot was harrowed the beginning of June. The second ploughing was in July by hired teams; it was harrowed the second time in the beginning of September; limed with 3250 bushels of lime, allowing about 270 bushels to each acre. It was sown in the middle of October with 40 bushels of wheat, and harrowed. The wheat plants are come up in abundance, and look as healthy and promising as wheat on any lands, in the country.

It has already been remarked, that upon these light loose soils, it is necessary to sow a larger than usual quantity of seed by the acre. The soil is here about nine inches deep, but remarkably loose and light. The substratum above the sand rock is the same rammel or cat brain, which is found under most of these wastes.

4. I obtained from another landlord a lease for 21 years of two other allotments, amounting together to 20 acres. By the terms of my lease I am to pay no rent for the first seven years; for the remaining fourteen years the rent is to be 14*s*. an acre. The tenant is to make the fences at his own expense.

Sixty roods of the fence round the first of these two lots, which consists of 12 acres, is a stone wall six feet high; 30 roods, a strong ditch and bank faced, as in the last lot, with stone, and protected above with posts and a single rail. On the summit of the bank hawthorn quick is planted. A single rail was here thought sufficient, because it is a fence between two closes, not been a close and the road.

The first ploughing was in September 1804; this was the first lot after the Rhos above mentioned, upon which I employed my own teams. It was harrowed early May 1805; cross ploughed in the beginning of June; harrowed immediately; limed in the same month with 3250

Improvement of waste lands.

bushels of lime, the same quantity as in the last mentioned lot; ploughed a third time, and sown in the end of October, and harrowed. The quantity of wheat sown, as in the last lot, was 40 bushels. The soil is here partly sand upon the sand rock, and partly a light soil upon rammel. Hand labour was employed at a great expense upon a stony part of this lot, in quantity about three acres. The wheat plants upon this lot are of as promising an appearance as upon either of those above described.

5. The other lot of eight acres, obtained by the last mentioned lease, is not yet enclosed. The labourers are now (13th January) employed upon the fence. It was ploughed in January 1805, and harrowed in the same month. It is now a fallow intended for pease. This lot would have been prepared for wheat and sown; but lime, in sufficient quantities for all my improvements, could not this year be obtained, at the only rock from which it could be conveyed at any reasonable expense to these lands.

I was induced to offer the rents above stated of 10*s.* and 14*s.* an acre (in the last case, the land to be for seven years rent free) because I was confident, that these wastes were capable of improvement. But in the natural state in which I found them, they were not worth 2*s.* an acre. They afforded pasture to a few half-starved sheep of the worst Welsh breed; and the sheep did more damage to the fences of the old enclosed lands in winter, and to the lands themselves, than could be compensated for by the profits which their owners derived from them.

The closes now fenced and improved, are well worth a guinea an acre. A year ago they were not worth two shillings.

The improvements upon the first four lots, above described, are to a certain degree complete. They contain fifty-three acres of as fine green wheat, as any which this country contains.

6. A sixth close of thirty-two acres, allotted to me as proprietor, is fenced with a wall six feet high, and one hundred and ten roods in length: most part of the wall is pointed with mortar on the outside; the rest is to be pointed

pointed next summer. The lower part of this allotment is bounded by the fences of my old enclosed lands. On the exposed side, towards the N. W. a plantation is intended, fifty roods in length, and twelve yards in breadth. Improvement of waste lands.

This close was almost entirely covered with gorse. There was, as I stated above, much of this plant upon the lands already described. My first operation was to stock up the gorse. I gave my labourers three guineas for this work, upon this lot only. They were also to have the gorse for their own use, which was partly used for fuel, and partly sold by them. They sold it at 5*s*. the cart load. I made an experiment upon five acres of this close, where a plough could not at first be used. After the gorse was stocked off, the land was pared and burned; and the ashes were spread. The plough could, after the land had been thus treated, though with some difficulty, be used. I ploughed it in June, 1805; harrowed it; ploughed it three times more; and sowed it, about the end of the same month, with turnip seed. There is now upon the land a fair average crop of turnips.

I also pared three acres more. Part of this was burned, and part was manured with dung. Where the dung was laid, the ground was trenched about nine inches deep; the sod was placed with the surface downwards within the trench. The dung was laid in moderate quantities upon the sod, and covered with about six inches of soil. Potatoes were then set in the beginning of May in rows. They were hoed twice. The produce was abundant.

The remaining 24 acres of this inclosure, were ploughed in February and March, 1805. Fourteen acres were, after one ploughing, sown with 71 bushels of black oats; 11 acres with pease; and three acres with summer vetches. The ground was then well harrowed. I had little land of my old enclosures this year in oats. It was my wish to try, whether a crop of this grain might be obtained, upon land so fresh and light as this, without manure, and with one ploughing. The first promise, however, of the oats, was so bad, when they began to appear above the ground, that I thought it best to throw some lime upon the land; which, if not so beneficial to the crop of oats, will be useful

Improvement of waste lands.

ful to any succeeding crop. I therefore manured 18 acres: viz. all the land where the oats were sown, and part of that sown with pease, with 4550 bushels of lime.

I obtained from the 71 bushels of oats, a return of, 360 bushels; a clean, thin crop, intermixed, indeed, with a little fern. The pease and vetches produced but a poor return.

In the beginning of November my teams were not much employed. I sent them to try how this land would appear when ploughed up. I found the part which had been limed remarkably mellow. I conceive that this favourable appearance, arose from the length of time that the lime had been upon the ground. I then procured several hired teams in addition to my own. It was all ploughed up by the twentieth of the same month; sown with 95 bushels of wheat, and harrowed. The potatoe land was sown with wheat, at the same time. The six acres, which had not been limed, are to be manured with 240 bushels of soot. The soot is now in waggons upon the ground; and the first favourable day, it will be thrown upon the land as a top dressing.

This is the last field sown by me. The wheat plants are now (January 13) making their appearance above ground, and look well.

I intend this year to proceed with activity in the improvement of the following allotments, which still lie waste.

7. and 8. One of these lots of 25 acres I obtained in exchange for four acres and a half of old enclosed, arable land, detached from my farm, of much the same quality with other arable lands in this neighbourhood. This circumstance alone proved of how little value these waste lands were. These 25 acres were an object to me, as they lay contiguous to another of my allotments; and they are equal in goodness of soil to any upon this hill. After this land has undergone the process described in lots 2d and 3d, I hope to see these 25 acres of equal value, acre by acre, to the four and a half which I gave for them.

These 25 acres, as well as lot 8, 27 acres, allotted to me as proprietor, will be improved next summer.

9. Three

9. Three acres of steep ground, that can never be cultivated, will be planted, this spring, with different kinds of forest trees. Improvement of waste lands.

The tables of expenditure are below. The return in the first lot has more than repaid all my expences the first year; and the return promised by the three succeeding lots, is little less abundant.

If I am permitted to live another year, and to enjoy my usual health, I hope to see 148 acres of land, which was so lately almost entirely unproductive, covered with golden harvests, or adorned with thriving plantations.

LOT I.—8 *Acres.*

	1804.	£.	s.	d.
64	roods of fencing, at 7*s.* per rood - -	22	8	0
1	gate and posts - - - - -	1	1	0
1690	Winchester bushels of lime, at 2½*d.* per bu.	17	12	1
	Carriage - - - - - -	5	4	0
	1st ploughing, 20*s.* per acre - -	8	0	0
	2d ditto 15*s.* per do. - -	6	0	0
	3d ditto 10*s.* per do. - -	4	0	0
	4th ditto 10*s.* per do. - -	4	0	0
3	separate harrowings, at 15*s.* per acre—5 acres	6	0	0
24	Winchester bushels of wheat, at 11*s.* do.	13	4	0
	Labour - - - - - -	1	10	0
	1805.			
1	ploughing and harrowing, at 15*s.* per acre	6	0	0
24	Winchester bushels of wheat, at 11*s.* -	13	4	0

LOT II.—21 *Acres, Fenced by Landlord.*

85½	roods of stone walling, at 17*s.* per rood	72	13	4
3	gates, at 1*l.* 1*s.* each - - - -	3	3	0
5200	Winchester bushels of lime, at 2½*d.* per bus.	54	3	4
	Carriage - - - - - -	10	0	0
	1st ploughing at 20*s.* per acre - -	21	0	0
	2d ditto at 15*s.* per do. - -	15	15	0
	3d ditto at 10*s.* per do. - -	10	10	0
3	separate harrowings, at 15*s.* per do. -	15	15	0
71	Winchester bushels of wheat, at 11*s.* per b.	39	1	0
	Labourers for stocking, levelling, clearing stones, and spreading lime - -	15	5	2

LOT

Improvement of waste lands.

LOT III.—12 *Acres*, *Fenced by Landlord.*

26½	roods of stone walling, 6 feet high, at 17*s.* per rood - - - -	22	10	6
67½	roods of fencing, with stone face, and double posts and rails, and hawthorn quick, at 10*s.* per rood - - -	33	15	0
58	roods of sunk fence, 5 feet deep, and staggers on the top along the dyke, at 4*s.* 9*d.* per rood - - - -	13	19	6
2	gates and posts, at 1*l.* 1*s.* each - -	2	2	0
	1st ploughing, at 20*s.* per acre - -	12	0	0
	2d ditto at 15*s.* per do. - -	9	0	0
	3d ditto at 10*s.* per do. - -	6	0	0
3	separate harrowings, at 15*s.* per acre -	9	0	0
3250	Winchester bushels of lime, at 2½*d.* per bushel - - - - - -	33	17	1
	Carriage - - - - - -	6	5	0
40	Winchester bushels of wheat, at 11*s.* per bushel - - - - - -	22	0	0
	Labourers, for stocking, levelling, and spreading lime - - - -	8	3	0

LOT IV.—12 *Acres*, *Fenced by Tenant.*

60	roods of stone walling, at 15*s.* per rood	45	0	0
30	ditto of fencing, with single posts and rails on top, with hawthorn quick, at 9*s.* 4*d.* per rood - - - -	14	0	0
2	gates and posts, at 1*l.* 1*s.* - - -	2	2	0
	1st ploughing, at 20*s.* per acre - -	12	0	0
	2d ditto at 15*s.* per do. - -	9	0	0
	3d ditto at 10*s.* per do. - -	6	0	0
3	separate harrowings, at 15*s.* per do. -	9	0	0
3250	Winchester bushels of lime, at 2½*d.* per bushel - - - - - -	33	17	1
	Carriage - - - - - -	6	5	0
40	Winchester bushels of wheat, at 11*s.* per bushel - - - - - -	22	0	0
	Labourers, stocking, levelling, clearing, and spreading lime - - - -	14	8	6

LOT

LOT VI.—32 *Acres.*

Improvement of waste lands.

110	roods of stone walling, 6 feet high, at 17*s.* per rood - - - - - -	93	10	0
6	ditto of fencing, at 9*s.* 4*d.* per rood -	2	16	0
3	gates and posts, at 1*l.* 1*s.* each - -	3	3	0
	1st ploughing, at 20*s.* per acre - -	32	0	0
	2d ditto at 15*s.* per do. - -	24	0	0
2	separate harrowings, at 10*s.* per acre -	16	0	0
4550	Winchester bushels of lime, at $2\frac{1}{2}d.$ per bushel - - - - - - -	47	7	11
	Carriage - - - - - - -	8	15	0
71	bushels of oats, at 3*s.* 4*d.* per bushel -	12	7	0
24	ditto of pease, at 5*s.* per bushel - -	6	0	0
8	ditto of summer vetches, at 6*s.* per bushel	2	8	0
240	ditto of soot, at 6*d.* per bushel - -	6	0	0
	Sowing the same, at 5*s.* per 100 bushels	0	12	0
95	Winchester bushels of wheat, at 11*s.* per bushel - - - - - - -	52	5	0
	Paring and burning 5 acres, for turnips, at 1*l.* 16*s.* per acre - - - -	9	0	0
2	extra ploughings, at 20*s.* per acre -	5	0	0
2	ditto harrowings, at 10*s.* per do. -	2	10	0
	Spreading the ashes, at 2*s.* 6*d.* per do. -	0	12	6
5	pounds of turnip seed, at 1*s.* per pound	0	5	0
2	hoeings, at 7*s.* per acre - - -	1	15	0
150	roods of 8 yards square of stocking, at 1*s.* per rood, for potatoes - -	7	10	0
	Paring 3 acres, at 1*l.* 6*s.* per acre -	3	18	0
	Labourers for clearing the gorse - -	3	3	0
	Ditto levelling and spreading the lime -	5	15	0

LOT V.—8 *Acres, a Fallow for Pease; One Acre Waste.*

	1st ploughing, at 20*s.* per acre - -	7	0	0
	Harrowing, at 5*s.* per do. - - -	1	15	0
	Total amount £.	1073	1	0

SCIENTIFIC

SCIENTIFIC NEWS.

On the Tempest of Feb. 18, *which has produced many dreadful accidents in the Channel.*

Observations by La Marck on his system concerning the influence of the moon upcn the weather.

THE interesting nature of the observations I am about to communicate, appears to me to be of too serious an importance to permit any consideration to delay their publication, to which I wish to give the greatest authenticity.

I have been long convinced by observation, that many points in the course of the moon have unquestionable influence on the atmosphere, although the causes which modify these influences are not sufficiently appreciated to enable us to predict what events may be expected at those periods.

I add, that the results of my observations, recently completed, has strongly confirmed my opinion in this respect, and has informed me, that, independently of the influences of the *syzigies*, the *quadratures*, and the two *apsides* the *nodes* of the moon have very remarkable influence, but more powerful in some particular cases, as I have succeeded in ascertaining.

Of 311 *nodes* and *contra-nodes* marked in my collecton of Observations, 177 have eminently distinguished their influence; 134 have manifested no particular power. The difference is 43 in favour of the influence of these lunar points. But I observe that the *contra-nodes* have somewhat more power than the *nodes*, and that especially the power of those *contra-nodes* which occur during the half-yearly period of the sun's being north of the line, deserve the most serious attention. There are even circumstances wherein I find that the evil influence of the *contra-nodes* has never failed to shew itself. I shall describe them, as well us the details of my recorded observations, in the next *Annuare Météorologique*.

But it is of consequence that I should explain to the public, that the tempest of the 18th of February last is the resnlt of a *contra-node* which took place the evening before, under circumstances which I promise to explain.

(To be continued.)

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

JULY, 1807.

ARTICLE I.

Facts toward a History of Prussiates. *By* Mr. PROUST.*

THE Prussian blue of the shops is seldom pure, as Scheele had already observed. Frequently, beside the alumine which makes a part of it, we find silex, carbonate and sulphate of lime, sulphate of potash, phosphate and red oxide of iron, sulphur, ammonia not divested of animal oil, &c. To study the nature of this combination therefore, it is indispensable, to use only a prussiate free from alum, and sufficiently edulcorated with acids and boiling water.—It even appears, according to a remark of Berthollet, that the prussiate of potash can attach itself to the Prussian blue so forcibly as to resist ablution to a certain degree. I do not think with him however, that the surcharge of this salt should be considered as an element essential to it; for when the blue has been well prepared, and such is to be met with in the shops, it leaves no trace of saline matter in the residuum after distillation.

Impurities of Prussian blue.

Prussiate of potash not essential to it, as Berthollet supposed.

Prussian blue prepared without alum has a coppery appearance like the best indigo. It loses only forty-five *per cent* by combustion. Its residuum is red oxide of iron without any mixture of foreign matter.

Pure Prussian blue.

* Annales de Chimie, Vol. LX. p. 185. Nov. 1806.

Action of Alkalis.

Treated with caustic potash,

The blue passed through caustic potash leaves a residuum, which is nothing but red oxide blended with alumine. Its colour is that of kermes, if the blue were of good quality; on the contrary it is pale and earthy, if the blue were surcharged with alumine: so that we may form a pretty good judgment of its nature by the colour of its residuum.

may be deprived of all its acid at once.

Acids acquire no colour from the residuum properly washed; which shows, that Prussian blue may be deprived of all its acid at a single operation; but for this it must have been very finely pulverized, which is attended with some difficulty. If a few drops of alkali be added to water coloured by the blue recently precipitated, it will be deprived of its colour completely; and in this case the oxide separated from it will not afford the least trace of colour, when it is wetted with an acid.

This seldom the case.

In the process followed it frequently happens that the ochry residuum retains either some remains of blue, that have not been touched by the alkali, or a mixture of prussiate of potash and ferruginous alkaline carbonate, or all three of these blended together. I shall proceed to examine two of these cases, and it will be easy to form a judgment of the third.

Blue left in the residuum.

If for example an acid be applied to a well washed residuum, which still retains Prussian blue, this blue will not discover itself in pulverulent particles, but in proportion as the acid frees it from yellow oxide. There is no particular chemical union between this oxide and Prussian blue, as hitherto has been supposed; at least we have no positive indication, that the metallic salt, called prussiate of iron, is susceptible, like so many others, of a *maximum* and *minimum* state either of acid or of oxide; and if the mixture of yellow and blue, which these residuums sometimes offer us, be not green, as might be expected, it is because the yellow oxide always covers these remains of blue in very great excess; at least I have never found the blue to be above one or two hundredth parts.

Prussiate of potash, and ferruginous alkaline carbonate,

I proceed to the second case. A residuum may contain no remains of blue, if it were well pulverized, but it easily retains the two salts I have mentioned above. If an acid be then applied to it, each of them affords abundance of

blue. We shall examine the particular mixture of these two salts farther on: but if the residuum have been washed with care, acids will not give rise to more blue. This washing is very tedious, it is true; for I have been obliged to pour boiling water at least twenty times following on a single drachm of residuum, before I could obtain it completely free; but when this is at length accomplished, acids will dissolve it, without any blue being produced.

not easily removed by washing.

When these residuums effervesce with acids, they contain carbonate of potash or of lime. The former may be carried off by ablution; and the second will be detected by vinegar after the washing. It is not the red oxide therefore, that occasions this effervescence: it cannot indeed combine with the carbonic acid, consequently cannot take it from the potash, in exchange for the prussic acid, which it cedes to it. In nature, as in art, it is only the oxide of iron at a minimum, that can combine with the carbonic acid.

Effervescence indicates a carbonate.

Red oxide of iron does not unite with carbonic acid.

A pound of Prussian blue of the shops of fine quality, has afforded as much as nine ounces and half of crystallized prussiate of potash. It is by no means uncommon, to find in the mother water left to itself truncated octaedra an inch in diameter. If the blue be contaminated with sulphuric acid, at least four crystallizations will be requisite, to free the prussiate entirely from sulphate of potash. The mother water contains alumine, sometimes in abundance, sulphate and phosphate of potash, ferruginous alkaline carbonate, &c. Hence may be inferred the importance of employing crystallized prussiate of potash in analyses, and not simple lixivia of Prussian blue, as was formerly done. Prussiate of potash is unalterable in the air, whether dry or damp; boiling it for any length of time does not alter its nature: its taste is sweetish, and slightly saline, leaving after it a faint impression of bitterness. It is insoluble in alcohol; which separates it from its aqueous solution in a white snow with the lustre of mother-of-pearl; and this lustre it retains when dry, so that it might be mistaken for the argentine gauze of acetate of mercury. Redissolved in water it reproduces a common solution of triple prussiate.

Prussian blue affords $\frac{19}{32}$ of prussiate of potash.

Lixivia not pure,

the crystals therefore should be used as a test.

Characters of the prussiate,

This salt, which I shall term a triple salt, or trisule, indiscriminately, to distinguish it from the simple prussiate of

which is a triple salt,

potash, is as constant in its qualities as the most perfect neutral salts. It is of a fine lemon colour, which it never loses without changing its state. For this, as well as for its other two characteristic properties, that of crystallizing, and of changing the red oxide of iron blue, it is indebted to a portion of black oxide, which is essential to its constitution. Without this oxide, confined like the other two elements of the triple prussiate to an invariable proportion, this prussiate in fact could neither crystallize, nor form blue with solutions of iron, the base of which is at a maximum of oxidation. In short, it is from this very union, that the principle which saturates the potash of the triple salt derives those properties, as Berthollet remarks, that singularly increase the analogies it bears to acids.

Containing black oxide of iron.

This gives the prussic acid more decidedly acid properties.

In this point of view we may add, that the triple prussiate occupies a mean betwixt alkaline and metallic salts.—However, when we reflect on one property of this salt, which will be mentioned below, it is difficult to say, whether it be to the prussic acid simply, or to the combination of this acid and potash, that the oxide of iron attaches itself, when it converts the prussiate into a triple salt. Thus much is certain, that we do not yet by any means know what appearance or properties a prussic acid might have, that should be combined with the precise dose of black oxide, by means of which it can furnish a triple prussiate. By treating this oxide with prussic acid we can form Prussian blue, but not that kind of ferruginous acid, which is capable of converting potash into a triple salt. Of this we must not lose sight; for it is well known, that Prussian blue is not of a nature to combine with potash without leaving a residuum. In short, the triple prussiate divested of its alkaline base, if I may so say, is a compound, which no fact, no appearance authorizes us to consider rather as a salt, the acid of which has been particularly exalted by its union with the oxide, than as a combination perfected altogether by this oxide.

This combination of black oxide and prussic acid not obtainable by itself.

One property, which appears in fact to militate against our admitting the prussiate as a salt, the acid of which is exclusively united to the black oxide, is that of its resisting the action of alkaline hidrosulphurets. If these reagents, which

Apparently a joint affinity between the acid, oxide, and alkali.

which spare no other known metallic salt, have no action on the triple prussiate, we are to a certain degree justified in presuming, that the oxide of iron is not exclusively attached to the acid of the triple prussiate; unless indeed we suppose, that the affinity of this acid for the oxide be so great, as to defend it from the common fate of all other oxides. We shall see however farther on, that an affinity so extraordinary, unexampled as it has hitherto been in chemistry, is not impossible. I proceed to the trial of the hidrosulphuret of potash on the triple prussiate.

Hidrosulphuret and Triple Prussiate.

The hidrosulphuret of potash or of ammonia, even assisted by heat, has no action on this salt. If it contained any remains of ferruginous carbonate, it would be freed from them, for the hidrosulphuret decomposes this carbonate. It may be filtered, if necessary, and the prussiate will nevertheless crystallize in its usual form. Such a result leads us to acknowledge, as has been hinted above, a very peculiar intimate combination between the three elements of the triple prussiate. But we shall presently see these very hidrosulphurets contribute to our obtaining the white prussiate in all its purity, or that combination in which the iron is at a minimum state of oxidation, which I made known in my former memoir on Prussian blue.

The triple prussiate not decomposed by hidrosulphurets.

White Prussiate.

Over a lamp place a matrass containing fifteen or eighteen grains of prussiate of potash, and two or three ounces of hidrosulphuretted water. A few seconds after the ebullition and vapour have expelled the air, that occupied the upper part of the matrass, drop in slowly a very dilute solution of green sulphate of iron from a phial, into which a few grains of sulphuret of the same metal have previously been put, in order to keep its base at a minimum of oxidation. Immediately a precipitate will be formed, rendering the liquor as white as milk, and so it will remain as long as the heat is kept up. This is the precipitate which I call white prussiate, and is the same as has been obtained by Fourcroy, Vauquelin, Davy, and no doubt all, who, paying

White prussiate of iron.

ing attention to the conditions necessary to ensure success, have found, that the base of the green sulphate might become that of a prussiate different from the prussiate which has for its base oxide at a maximum. But as the black oxide, in passing from one combination to another, never loses its disposition to acquire a surplus of oxigen, we perceive, as soon as the matrass is removed from the fire, that the atmosphere reacts on the milky mixture, and rapidly produces tints of colour, which gradually diffuse over the whole a fine deep blue.

Rendered blue by oxigen from the atmosphere.

This product may be obtained in another way. Let fall prussiate of potash grain by grain into a very dilute boiling solution of green sulphate, and a precipitate will make its appearance, the whiteness of which will resist the action of the air somewhat longer.

Another method of obtaining white prussiate.

I shall add a few other processes, which, if they do not increase conviction, may be interesting from the variety of the means employed.

Fill two glasses, one with nitrate of iron, and the other with green sulphate, each in a state of very dilute solution; and drop into each a crystal of prussiate of potash. In the former we shall see the crystal instantly coloured of so deep a blue, that it resembles black velvet. In the latter it cracks, separates, and falls into a white powder: but, as it had imbibed atmospheric air previous to the experiment, the precipitate is variegated like sage cheese.

A third.

Let two glasses be filled with boiling water; add to each a few drops of prussiate of potash, and to one of them a few drops of hidrosulphuret of potash, or of ammonia, likewise. Let fall into each a few drops of nitrate of iron; and the former, as might be expected, will give a complete blue; but the latter will exhibit the amusing appearance of a precipitate, which, at first blue, will rapidly lose its colour, and become white. The theory of these facts is so obvious, that I shall pass it over: neither shall I here repeat all the other experiments given in my first paper to establish the existence of two prussiates of iron. If the prussiate at a minimum have no colour when it is not acted upon by the atmosphere, we see, that the dried green sulphate is equally colourless. The absence of colour in one of these salts is surely

A fourth.

White prus-

surely not more surprising than in the other; and, if we obtain red oxide by applying alkalis to a blue prussiate, on the contrary we obtain black oxide from a white prussiate. But these differences, which might be inferred from theory, agree perfectly with those exhibited by the red and green sulphates under similar circumstances.

siate gives black oxide on being precipitated.

In my first paper I directed to pour the prussiate of pot-ash on the sulphate in a phial, to avoid as much as possible the admixture of air; but this succeeded imperfectly; first because cold liquors always have air in them, and secondly because I had not thought of sulphuretted hidrogen to free them from it. I did not then know how it acted with respect to these salts.

Caution.

If for instance a solution of green sulphate be diluted with three or four times its bulk of sulphuric or muriatic acid, the excess of these acids makes no alteration in the result. As the white prussiate wants colour only from defect of oxigen, it may be supposed such an addition is not calculated to impart it. These concentrated acids may alter the whiteness of the prussiate indeed, but they can never bring it to a complete blue.

Excess of acids do not change the white prussiate to the blue as they do not supply the necessary oxigen.

Muriatic acid boiled on white prussiate is equally ineffectual.

Not that this boiling acid is without action on the white prussiate; for I have observed, that there is some white prussiate destroyed, prussic gas evolved, and black oxide found in solution. The little Prussian blue, that is formed by the introduction of air, during the interval of these mixtures, predominates over the white, and changes its colour to a greenish.

Action of muriatic acid on it,

The blue prussiate boiled with the same acid likewise gives out prussic gas, and parts with red oxide, but less of it is destroyed than of the white prussiate. From these facts we may infer, that the muriatic acid, assisted by heat, is capable, in strictness, of decomposing prussiates, and assuming its rights of a stronger acid over the prussic; which would not be at all surprising, but at least I believe it would require considerable time.

and on the blue prussiate.

Prussiate

Prussiate of Potash and Acids.

The crystallized prussiate treated with acids.

Let weak sulphuric acid or muriatic be heated in a matrass with crystals of prussiate of potash. When ebullition commences, gas escapes, and may be received under a jar over mercury, or burned by applying to it a lighted candle. The flame will be variegated with red, violet, and yellow; and during the extrication of the gas the liquor will be thickened by the production of a white precipitate, which changes to a blueish. When all the gas is evolved, throw the mixture into boiling water, brighten it with oxigenized muriatic acid, wash, and dry the product in a capsule. Four experiments, made at different times, afforded me thirty-four or thirty-five parts of complete blue from a hundred of the triple prussiate.

It affords ·34 or ·35 of Prussian blue.

I proceed to the consequences:

Prussian blue contains ·54 or ·55 of red oxide of iron.

A hundred parts of Prussian blue, without alum, yield fifty-five of red oxide by combustion. The same blue, destroyed by nitric acid, gives fifty-four. It cannot be questioned therefore, that Prussian blue contains fifty-four or fifty-five hundredths of red oxide. From these data thirty-five parts of blue ought to produce about seventeen of black oxide, or nineteen of red. Hence it follows, that, when iron was formerly separated from a solution by prussiate of potash, this salt added to the product the nineteen hundredths of red oxide arising from its own decomposition; but the addition was still greater, when a simple alkaline lixivium of Prussian blue was used instead of the crystallized prussiate. The reason of this we shall see presently.

The prussiate therefore, used as a test, adds 19 per cent to the oxide thrown down.

Prussian blue treated with common potash.

When Prussian blue is treated with a common lixivium of potash, part of the alkaline carbonate loads itself with red oxide; and the result is a solution answering to Stahl's martial tincture, of which pure potash is insusceptible.—This solution, which may be prepared likewise by adding a few drops of nitrate of iron to a solution of carbonate of potash, will not occasion the least change in prussiate of potash, even by standing together. It is the same ferruginous carbonate, which, as I have said, is found in the mother-water. In effect, if an acid be added to the mixture of these salts, a perfect blue is precipitated, because the

new

new solution of oxide, which takes the place of the ferruginous carbonate, decomposes the prussiate of potash, as any solution of iron would do.*

When Prussian lixivium therefore is employed instead of a crystallized prussiate in an analysis, we add to the product in the first place red oxide, which made part of the ferruginous carbonate; and in the next black oxide, which is a constant element of the triple prussiate contained in the lixivium.

Inaccuracy of the Prussian lixivium as a test.

Chemists very soon discovered the faults of these lixiviums, though they were not at first aware, that they contained two very different combinations of iron, the carbonate of which I am speaking, and the triple prussiate. Many, on seeing the blue they yielded with acids, even thought this blue existed in them as a distinct substance; and endeavoured to precipitate it, whether it were Prussian blue or oxide, not affecting the alkaline prussiate, which they supposed to possess the tinging property without being indebted for it to iron. From their attempts arose the receipts for precipitated lixiviums, which occur in every work on chemistry. But since the inquiries of Scheele and Berthollet, it has been found, that these receipts answer the end but imperfectly; for it is easy to see, that it was not sufficient to free a lixivium from the oxide introduced into it by the carbonate; there remained farther to be guarded against the black oxide, which belongs to the triple prussiate, and the existence of which was the less suspected, because the addition of acids, without the intervention of light or heat, could not render the products of its decomposition perceptible.

Attempts to correct it.

I shall not stop to analyse the phenomena, that presented themselves during the preparation of lixivia either hot or cold, because, as the inutility of prussiates for the evaluation of iron in analyses is now well known, the particulars would not be very interesting. In the same manner I shall pass over the proposed test liquors with ammonia, lime, magnesia, &c. because they are themselves triple prussiates,

Other prussiates as inaccurate, without a counterproof.

* It is the mixture of these same salts, which enables the mother-waters of soda to afford Prussian blue by the addition of an acid.

Prussian blue from mother water of soda.

on

on which consequently we cannot rely, unless we join with them the counterproof proposed by Berthollet. I shall only add, because this should remain consigned to the history of the science, that, when a chemist still employs with effect a lixivium, or test liquor, purified by an acid, we may be certain, he has not obtained a complete and entire separation of the iron, as he flatters himself: for it is certain, that every lixivium capable of affording blue with a solution of red oxide contains black oxide, since without the assistance of this oxide there would be no tingeing prussiate; in other words, every prussiate of potash, not rendered a triple salt by black oxide, is incapable of forming blue with a solution of iron, the oxide of which is at a maximum, as is most commonly the case with those produced in analyses. This is a fact which Scheele perfectly developed. We may conclude therefore, that the alkaline or earthy triple prussiates can no longer be considered as useful in analysis, any farther than litmus, galls, and other reagents, which merely indicate the presence of a certain principle, without being capable of ascertaining its quantity.

No prussiate will produce Prussian blue, unless black oxide of iron be present.

Prussiates of no farther use than to indicate the presence of iron.

Action of sulphuric acid.

The aqueous sulphuric acid, applied to the triple prussiate, affords the same results as the muriatic. A hundred parts of prussiate produce a hundred and fifteen or a hundred and sixteen of sulphate of potash. If we knew exactly how much alkali the sulphate contained, we might thence deduce the quantity of the base of the prussiate. A hundred parts of crystals of prussiate lose ten of water by distillation.

Boiling necessary,

To complete its decomposition by the acids, it must be kept boiling at least half an hour, in order to dissipate the gas entirely, and obtain the entire separation of the white prussiate, which is formed during the operation.

or the assistance of light.

The prussiate of potash dissolves cold in the muriatic acid, without being decomposed. This mixture, as Berthollet found, requires the assistance either of light, or of heat.

Action of vinegar.

Vinegar, assisted by boiling, decomposes it also: prussic gas escapes, and white prussiate is formed, which does not change blue so quickly as with the preceding acids: finally, this prussiate, which does not appear till the moment

ment when the ternary combination begins to dissolve, confirms by its whiteness the fact, that it is only the oxide at a minimum, which has the privilege of entering into the formation of the triple prussiate. This is one of those truths, on which Scheele left nothing to be desired; yet the distinction of the oxides in this substance is a point, to which subsequent chemists have not paid all the attention it deserves.

The black oxide of iron only forms a triple prussiate.

Black Oxide, an Element of Prussian Blue.

We have just shown, that this oxide, in a constant proportion, is essential to the constitution of the triple prussiate; but there is another object, that has also some claim to attention, which is, that this oxide is capable of following the prussic acid from one combination to another, without changing its state; that it can pass from prussiate to prussiate and back again, and even circulate through the most oxiding mediums, without losing the state of a minimum oxide: and this I conceive to be a point of view, which has been overlooked in the history of prussiates.

It is one essential principle of prussian blue.

If, for instance, we may say with truth, that the prussiate of potash would be neither yellow, nor crystallizable, nor tingeing; we may assert with equal foundation, that neither would prussian blue be formed without the intervention of this oxide: and in fact, when we make prussian blue with a solution of red oxide and of triple prussiate of potash, the black oxide in the latter salt enters into the new combination jointly with its acid; whence it follows, that this oxide, which is an element of the triple prussiate of potash, becomes so afterward of prussian blue; and even, as will be seen presently, of all the other metallic prussiates, that are made with this salt.

Resists farther oxigenation when thus combined.

This black oxide is so firmly intermixed in the compound of prussian blue, and so well defended from all farther oxigenation by its union with the prussic acid, that we never fail to find it again in this blue such as it was in the triple prussiate of potash. I will say more; if we make the blue with this prussiate and the green sulphate of iron, the oxide of the latter will be raised, as is well known, to its maximum, in proportion as the blue becomes coloured by

by the impression of the air: but it certainly will not be the same with the black oxide, which passes into the prussian blue jointly with the acid. This oxide will not lose the quality of being at a minimum, which it had in the prussiate of potash; that is to say, if, during the exposure to air, the basis of the green sulphate, and consequently that of the white prussiate, raise its proportion of oxigen from 28 to 48 per cent, the black oxide, the inseparable companion of the prussic acid, will not participate in this super-oxidation, but will invariably keep to its 28 per cent.

Neither the air, nor boiling nitric acid, nor oxigenized muriatic, oxide it more, except as these acids destroy the blue itself.

Not only the atmosphere, which so easily raises the bases of the sulphat, muriate, and white prussiate to their maximum, loses all its activity when applied to the black oxide in question; but neither boiling nitric acid, nor the oxigenized muriatic, can increase its oxidation. These acids are capable indeed of destroying prussian blue, and even reducing it to red oxide; but as long as any blue remains to be destroyed, this will to the last retain its black oxide in all its primitive integrity.

Red oxide of iron will not combine with prussic acid, but black will.

If red oxide be treated with prussic acid, no kind of combination will take place between them. This is agreeable to the observation of Scheele. But if we employ black oxide, we shall obtain a greenish prussiate, which will be rendered perfectly blue by the action of the air. Black oxide therefore enters into the composition of prussian blue. If this oxide were unnecessary, or if the red oxide might serve exclusively as the base of prussian blue, it does not appear why this oxide, brought into contact with the prussic acid, and even its solution mixed with simple prussiate of potash, should not afford prussian blue.

Affinity of the prussic acid for a given proportion of black oxide, very strong.

I have remarked above, that the affinity of the prussic acid for such a dose of black oxide, as adapts it to the production of the triple prussiate, may be sufficiently powerful, to protect it from the common fate of oxides combined with acids in general: and in fact it appears to me, that this inference may be drawn from the following experiments.

Experiment 1.

Pour hidrosulphuret of potash into a phial on prussian blue

blue, and keep the mixture closely stopped: at the expiration of a few days, the hidrosulphuret will be converted into a triple sulphate, and the red oxide of the prussian blue only changed into black hidrosulphuret. Whence it appears, that, if the red oxide have followed the example of all other oxides, when the hidrosulphuret finds them combined with acids, it is not the same with the black oxide.

Hidrosulphuretted water brings back prussian blue to the state of white prussiate, as it does the red sulphate to that of green sulphate. This is a fact which I made known in my first memoir, and the power of this reagent never goes farther; but the hidrosulphuret of potash completely changes the red and green sulphates into black hidrosulphuretted oxide. Why then cannot this hidrosulphuret extend its action to the black oxide in question? Certainly some singular affinity, of which I believe there are few instances in chemistry, enables the prussic acid, the weakest of all acids in so many respects, to protect this oxide against all the power of the alkaline hidrosulphurets. Experiment 2.

All the metallic solutions, that afford prussiates with the triple prussiate of potash, no doubt follow the example of those of iron. The prussiates resulting from it will retain in all its integrity the black oxide, which the prussic acid carries with it; but it is time to lay before the reader the capital experiment, which demonstrates, that prussian blue is a triple salt; and that the black oxide, which had passed from the triple prussiate of potash into the prussian blue, is capable of passing back again from the prussian blue to potash, without having for a moment quitted its state of a minimum oxide. This experiment I have no doubt is anticipated by every one, who has formed a clear idea of the triple prussiate of potash. The same in other prussiates.

Let us take, for instance, a prussian blue, which has experienced all the action that the atmosphere, or the most oxiding acids, can exert upon it. Let us apply to it pure potash, and we shall obtain a lixivium, which will yield only a triple prussiate, or that combination in which we find the prussic acid constantly united with the usual dose of black oxide. If this prussiate be really such as I have announced, and the reader will have no difficulty to believe, Proof that prussian blue is a triple salt.

there

there can be no objection I imagine to the theory that asserts, that the white or blue prussiates are triple combinations, as well as the prussiate of potash, which has concurred to form them.

Triple prussiate of manganese,

Prussiate of manganese being put into a solution of potash, the result was the crystallizable triple prussiate of potash, of a yellow colour, and containing its due proportion of black oxide. This prussiate of manganese then is a triple combination, containing the black oxide. The prussiate of copper of a sanguineous colour is no doubt another, for the simple prussiate of copper is yellow.

and of copper.

Scheele informs us, that other oxides also have the property of converting the simple prussiate of potash into a triple salt. This apparently opens a field to a series of researches, which are the more interesting, as they may lead to the discovery of some colour equally valuable with prussian blue; and lastly we may conclude, from all that has been said, that no simple prussiate of iron exists; a kind of combination however, of which other metals are susceptible, as will soon appear.

Perhaps other prussian pigments.

Simple prussiates of other metals.

Distillation of Prussian Blue.

Destructive distillation of prussian blue.

This prussiate is destroyed by exposure to a high temperature. The new products, that arise from it, confirm the theory Berthollet has given us respecting the nature of the prussic acid. We obtain an acid which escapes destruction, carbonate of ammonia, a little free carbonic acid, gaseous oxide in abundance. An ounce of good blue of the shops afforded rather more than five pints of this gas, with as much carbonic acid as made up the whole three quarts. The water of the trough contained prussic acid fixed by ammonia. This prussiate, as is well known, follows the steps of that of simple potash; it cannot produce blue with solutions of red oxide, but it does with those of oxides at a minimum, because at the same time it forms itself into a triple or tingeing prussiate.

The residuum weighed five drachms fifty-two grains. It was perfectly black, and very attractable by the magnet.

A pyrophorus.

It is a pyrophorus, which takes fire with rapidity. After it has been kept in a phial not closely stopped, so long that it will

will not kindle of itself, if it be wetted with nitric acid of 40°, it burns with great vividness. I am inclined to think, that in this combustion the iron burns in conjunction with the charcoal.

If the prussian blue were without alum, this residuum contains nothing but charcoal and iron. Residuum.

Muriatic acid disengages from it with the greatest facility that aromatic hidrogen, which announces iron steelified, or combined with carbon. The residuum is pure carbon, one of the elements of the acid destroyed. As to the carbonic acid and gaseous oxide, it is equally evident, that they are the two oxidations of carbon, a maximum and minimum, produced by the oxigen of the two oxides found in the prussian blue. Treated with muriatic acid.

This decomposition is obtained by a heat so gentle, that it appears to me a convenient mode of procuring the gaseous oxide of carbon. As there is not the slightest appearance of oil, it is somewhat surprising, that in the course of the destruction of a compound in which carbon and hidrogen abound, no part of these combustibles should be found to present themselves under circumstances in which oil would be formed. Gaseous oxide of carbon. No appearance of oil.

The oily and aromatic character, that the hidrogen assumes during the solution of the residuum, demonstrates likewise, that the combination of iron with carbon does not require a very high temperature. The charcoal of blood, which is obtained by a very low heat, equally contains iron in the state of carburet; for this likewise yields odoriferous hidrogen with muriatic acid. I think I have somewhere else observed, that Priestley was struck with the bituminous smell of the hidrogen furnished by carburetted iron. Iron unites with carbon at a moderate heat.

Distillation of the Triple Prussiate of Potash.

This salt loses ten per cent of water, and with it its colour, for it becomes white; but it does not begin to soften till it is at a red heat. Some chemists have imagined, that roasting or melting it would afford the means of freeing it from oxide, but the following results will show, that these processes lead to nothing useful. Destructive distillation of the triple prussiate of potash.

When

Progress of its decomposition by heat.

When this salt enters into fusion, a little prussic acid escapes, which is seized by the ammonia that is formed at the same time. Afterward a nebulous vapour rises, which condenses in the neck of the retort in a mealy powder. This vapour is not reproduced when the fusion is at an end. On examination the sublimate has the alkaline and bitter taste of simple prussiate.

Alcohol dissolves a portion of it, and what separates from it is triple prussiate unaltered: that is to say, this gives prussian blue with solutions of red oxide, while the other cannot.

If a lighted candle be applied to the mouth of the retort, the prussic acid burns alone; and the carbonic acid, arising from its combustion, forms with the ammonia crystals of ammoniacal carbonate, which condense in the neck of the retort a few lines beneath the flame. We will now proceed to examine the melted prussiate.

The mass resembles melted muriate of soda, is of an ashen gray, and strongly attracts moisture.

If we taste a bit of it, we find nothing of the sweetness of the triple prussiate, but an alkaline taste, flavoured with the bitterness of the kernels of stone fruit. This flavour announces, that there is simple prussiate of potash in this residuum. A few drops of acid extricate a gas, which does not belong to this prussiate, and which give a suspicion, that it contains carbonate of potash also.

Finally this mass, if set by to dissolve, deposits a black, micaceous, shining powder. On collecting it in a filter, it is found to be a mixture of charcoal, pure iron, and a little sulphuret of iron. The last is an accidental product. Its sulphur proceeds from the decomposition of the sulphate of potash, from which it is not easy to free the triple prussiate. This powder is obedient to the magnet. A weak acid disengages first sulphuretted hidrogen, then aromatic hidrogen, and at length nothing remains but charcoal powder.

Examination of the Solution of the Residuum.

The residuum examined.

If alcohol at 25° be mixed with it, immediately a shining pearly snow is formed, which may be collected on a filter.

Dissolved

Dissolved and crystallized, it affords yellowish crystals, of a sweetish taste, and furnishing prussic acid and white prussiate, when acted upon by muriatic acid. This is the prussiate freed from oxide proposed by Mr. Richter.

The alcoholic solution being distilled almost to dryness, and the residuum covered with alcohol at 30°, one portion is dissolved, and another falls to the bottom. The precipitate is found, on examination, to be carbonate of potash, with a remnant of the triple prussiate. The new solution being distilled affords simple prussiate, which is discoverable by its taste, and by its property of not producing blue with solutions of red oxide. These are the products I have found after subjecting the triple prussiate of potash to fusion.

Consequences.

Recapitulation of the results.

The triple prussiate cannot support a considerable temperature without being simplified in its composition. It frees itself from black oxide, and passes to the state of simple prussiate: but this too is reducible to something more simple, as we shall see below; and it then leaves in its stead potash and the usual results of the prussic acid, ammonia, and carbon. A portion of the latter serves to disoxigenize the black oxide, reducing it to iron, and forming carbonic acid with its oxigen.

During these changes, a part of the triple and simple prussiates escape being acted upon, in proportion no doubt as they become enveloped in the carbonate: but it is to be presumed, that a high and continued heat, in vessels capable of supporting it, would ultimately reduce these prussiates to two binary combinations, which are ammonia and carbonic acid, potash, iron, and some remains of carbon, that the oxigen of the iron and the water were incapable of acidifying.

Simple prussiate of Potash.

Mode of obtaining the simple prussiate of potash.

This is obtained by saturating potash, in Scheele's mode, with prussic acid disengaged from the prussiate of potash or of mercury. But a more expeditious way is keeping alcohol on a concentrated lixivium of animal coal, shaking

it from time to time; and the progress of the solution will be discovered by the alkaline and bitter taste of the alcohol. The lixiviums of charcoal of blood or of leather are seldom exempt from a little hidrosulphuret, because the sulphate that contaminates potash introduces sulphur into them. In this case it enters into the alcoholic solution; but the charcoal contributes to it likewise, for I have prepared lixivia with charcoal of blood and very pure carbonate of potash, and yet found hidrosulphuret in them, though in smaller quantity. It must not be forgotten indeed, that sulphur is found among the products of blood. It even appears, that, like phosphorus, it is capable of fixing in the charcoal, but not combined with the iron it contains; for the aromatic hidrogen, mentioned above, does not afford the least indication of sulphur by its smell.

Animal charcoal affords sulphur.

Characters of the simple prussiate.

The simple prussiate is easily recognized by its bitter alkaline taste, and the aromatic flavour with which it strongly perfumes the mouth. It precipitates solution of copper yellow, and does not afford blue with a solution of the red oxide of iron, but precipitates it of an ochry yellow, as a pure alkali would*. Finally, it affords blue with a solution of common sulphate of iron, because it first constitutes itself a triple prussiate, and afterward produces white or blue prussiate of iron. If the prussiate be black, it is because the alkaline hidrosulphuret introduces into it hidrosulphuretted oxide; but it may be freed from this by a few drops of acid, and the prussiate of iron will appear alone. The simple prussiate does not keep well unless closely stopped. Scheele has shown, that the carbonic acid is sufficient to separate the prussic from the

Blackness from hidrosulphuret removed by an acid.

Must be closely stopped.

* In a memoir on the stone of Sigena, I had mentioned this combination as possible, but it was from mistake. A sulphate of iron, which I had superexided by nitric acid, retained notwithstanding a portion of black oxide, and this deceived me; and Scheele, whom I contradicted on this point, saw more clearly than I.

In our abridgment of this memoir, Journ. Vol. XII. p. 2. we did not insert the remark here alluded to, as we were persuaded that Scheele was right, and that our author must have been deceived by some circumstance or other. T.

potash, their affinity being so weak. When it is not combined with black oxide, it will not crystallize by concentration, but fixes in a mass, in which however some saline laminæ are distinguishable.

This prussiate is the test liquor proposed by Scheele. Its utility in analysis is very confined; since all solutions, in which the iron is at a maximum of oxidation, and this is the most common case, are not in the least affected by this reagent, as he himself observed. To employ it with utility, part of the oxide of the solution must be brought back to a minimum, which is not easy, or to be done without risk of increasing the difficulties of the process. Of little use as a test.

Its Decomposition.

The aqueous solution of this prussiate gives out part of its acid at a boiling heat, which sufficiently demonstrates, that this combination is neither solid, nor comparable to any of those formed by oxigenized acids. It froths continually, and has something saponaceous. A lighted candle, applied to the orifice of the retort, sets that portion of acid on fire: but its loss is not confined to this, for that portion, which the salt retains more strongly by means of the potash that begins to predominate, likewise experiences a slow but regular destruction from the effect of the heat, which converts it into ammonia and carbonic acid. Examine the product at whatever period of the boiling you please, there will always be found in it carbonate of ammonia mixed with a little prussic acid; and at length, when the water begins to fail, this carbonate condenses in needles in the neck of the retort. Decomposed easily.

If water be supplied, that the boiling may continue, the same products will be found in the water of the receiver; but after four or five successive distillations in the same manner, they cease to be perceptible, though the saline residuum still evidently contains prussic acid.

On treating this residuum with alcohol, part is dissolved, which is found to be prussiate of potash; but the saline matter left undissolved is carbonate of potash. The two following experiments leave no doubt of the destruction of the simple prussiate by a boiling heat. The residuum.

Boiling converts the prussiate into a carbonate.

Prussiate of potash does not render turbid a solution of muriate of lime; but after it has undergone ebullition for some time, it precipitates it copiously in the state of carbonate. The prussiate of potash therefore must have been converted into carbonate of potash.

Experiment.

Two measures of solution of prussiate, one in its natural state, the other altered by long boiling, were employed to precipitate common sulphate of iron. Each afforded blue; but after the brightening, that produced by the former was three times as much as the other.

Decomposed by heat.

If dry simple prussiate be heated to redness, carbonate of ammonia will pass over, contaminated by an oily vapour similar to that of hartshorn. The saline mass being dissolved leaves behind charcoal, and is carbonate of potash mixed with a portion of prussiate not decomposed.

Consequences.

The simple prussiate a weak combination.

All these results unquestionably authorise us to conclude, that the simple prussiate of potash is a feeble combination, as Scheele had already found, the principles of which are easily dislodged, like all that are complex. We see in fact, that part of the acid separates from the potash by the effect of dilatation simply; while another part, subjected longer to the agency of caloric, is destroyed by being changed into ammonia and carbonic acid. Let us proceed to the application.

But not the triple prussiate.

That the triple prussiate of potash is not deranged by repeated ebullition is a fact. The lixiviums employed in manufacturing prussian blue contain, as we shall see below, both the triple prussiate and the simple prussiate. There is not found in them, however, any ammoniacal salt. It might be presumed, indeed, that the great excess of carbonate of potash they contain would be incompatible with such a salt; yet they evolve ammonia, as long as they continue in ebullition. Whence then can this ammonia proceed, if not from the decomposition of the simple prussiate? We may infer, therefore, that boiling the lixivia, or concentrating them by evaporation, is liable to injure them by the destruction of that very prussiate, which cannot be too sedulously preserved; and as the carbonate of

Boiling the lixivia in manufactories of prussian blue injurious,

potash

potash is likewise one of the principles resulting from this destruction, it does not cease to add to what is found there already.

Curaudeau was aware of the injury occasioned by boiling the lixiviums, and happily prevented its effects, by adding to them a little sulphate of iron, agreeably to the principle of Scheele, who made known, that the simple prussiate was converted into a triple prussiate, whenever it could acquire a portion of black oxide, and thus defended itself from decomposition. As to the products of the destruction of the prussiate by fusion, or by ebullition, undoubtedly there is nothing extraordinary in them, since it is sufficient for us to be acquainted with the nature of the prussic acid to foresee them; but it is not the same with respect to the carbonic acid, which presents itself during one of these destructions. Whence, for instance, comes the oxigen, which, during the ebullition of the aqueous prussiate, acidifies the carbon of the prussic acid? Either this oxigen must be one of the principles of the prussic acid that is destroyed, or we must suppose, that a decomposition of water has taken place. I do not think we are yet sufficiently advanced, to choose between these two opinions; but till we have a clearer insight into the subject, I cannot help saying, that, if we reflect on the circumstances accompanying the production of the prussic acid, we shall be more inclined to adopt the opinion of Berthollet, than any other hypothesis. His words are: "it appears to me difficult to conceive the existence of oxigen in a substance, which contains elements so strongly disposed to form particular combinations with it, as hidrogen and carbon, and yet is capable of enduring a pretty high temperature without being decomposed." In fact, to admit that this acid is an oxigenized compound, we must suppose, that such an acid is capable of disputing oxigen with the carbon by which it is surrounded on all sides; and not only place it at the head of the acids, but even of those oxides which are known to be most difficult of reduction.

unless a little sulphate of iron be added,

to form the triple prussiate.

Whence the carbonic acid formed?

Most probably no oxigen in prussic acid.

(To be continued.)

II. *A Table*

II. *A Table of the Growth of Trees in the Botanic Garden at Calcutta. By* Dr. Wm. Roxburgh.*

THE average circumfererence of several of each sort of trees mentioned in the list, in inches and quarters, was taken 4½ feet above the ground, at the end of October, when the year's growth may be reckoned over. The difference between the numbers in the adjoining columns gives one year's increase in the circumference of each sort.

Names of the Trees.	Synonymas.	Years Age of the Trees measured in 1803 and 1804.																		
		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Tectona grandis - -	Teak, E. Sagoon, H.	—	7	10½	—	—	—	—	18¼	21¼	—	—	—	—	—	42½	14¼			
†Swietenia Mahogani - -	Mahogany, E. - -	—	—	—	—	—	25½	30¼												
Swietenia chickrassa - -	Chickrassy, H. - -	—	—	—	—	—	—	—	25	28										
Swietenia febrifuga - -	Soymida, T. Roheena, H.	—	—	—	—	—	—	—	21½	24½										
Terminalia alata-tomentosa -	Asseen - - -	—	—	—	—	—	—	—	—	—	—	—	—	—	37	38				
Terminalia bialata - -	- - - - -	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	51	53½	
Terminalia catappa - -	Buddam - - -	—	—	—	—	—	—	—	—	30	32½									
Terminalia procera - -	- - - - -	—	—	—	—	—	—	—	23	25										
Terminalia belerica - -	Beheyrah, H. - -	—	—	—	—	—	—	—	—	20	23¼									
‡Dalbergia sissoo - -	Sissoo, H. - - -	5	11½	14¼	17½	—	—	—	—	—	—	—	—	—	—	—	—	—	70	75
Dalbergia ougienensis - -	- - - - -	—	—	—	—	—	12½	14¼												
§ Dalbergia emarginata -	- - - - -	—	—	—	—	—	—	—	—	—	—	—	—	25½	30					
Pterocarpus marsupium -	Yeangashaw, T. -	—	—	—	—	—	—	—	—	—	—	23	26							
Pterocarpus Dalbergioides -	Andaman, red-wood -	—	—	—	—	—	—	—	—	27	29½									
Cedrela toona - - -	Toon, or Tooni, H. -	—	—	—	—	—	—	—	—	—	—	32¼	35	—	42					
Mimosa seereesa - -	Seerees, H. - -	—	—	—	—	—	—	29	31½											
Mimosa Xylocarpa - -	Conda-tangheroo, T. -	—	—	—	—	—	17	21												
Mimosa elata - - -	- - - - -	—	—	—	—	—	—	—	—	28	30½									
Mimosa stipulacea - -	Amlooki - - -	—	—	—	—	—	—	40	48½											
Mimosa lucida - - -	- - - - -	—	—	—	—	—	—	—	—	—	—	49	56½							
Pterospermum paniculatum -	Mows-jal, H. - -	—	—	—	—	—	—	—	—	—	—	—	—	—	—	21½	24½			

Table continued.

Names of the Trees.	Synonymas.	Years Age of the Trees measured in 1803 and 1804.																		
		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Aleuritis triloba		—	—	—	—	—	28½	31												
Nauclea stipulacea		—	—	—	11	13½														
Nauclea nicotianifolia		8	15¾																	
Uvaria odorata		—	—	—	14	19														
Casuarina muricata, in garden mould		—	—	—	20	22														
The same in very sandy soil		—	—	—	20½															
Cassia Sumatrana		17½	23½																	
Butea frondosa	Palas, H.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	40	43
Lagerstromia Reginæ	Jaral, or Jarool, H.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	48½	53
Gmelinia arborea	Gumar, or Gumhar, H.	—	—	18½																
Melia azedarach		—	—	—	—	37½														
Bignonia suberosa		—	—	—	—	—	—	—	—	—	36	39¼								
Sterculia villosa		—	—	—	—	—	—	25	27½											
Andersonia altissima	Siri-Maun, T.	—	—	—	12½	15½														
Myristica aromatica	Nutmeg	—	—	—	—	3½	4½	Two feet and a half above the ground.												

* From the Transactions of the Society of Arts, for 1806.

† Two plants (and the first) were sent by the Honourable Court of Directors, to the Botanic Garden at Calcutta, in 1795, and by the end of 1804, above 500 plants had been reared from these two. It succeeds so perfectly well in the East Indies, that the tree will, without doubt, in a few years, prove a valuable acquisition to that country.

‡ An immense tree of very quick growth. It supplies the ship-builders at Calcutta, with all the knee, and other crooked timber. It is strong, but not very durable.

§ This promises to be more durable than Sissoo. It is a native of the Andaman Islands. As yet, the trees on the continent of India are small, and but few in number.

Note. In the column of Synonymas, H. means Hindoostanee, T. Telinga, and E. English.

The Specimens noted in the following Letter are placed in the Society's Repository.

DEAR SIR,

Ejoo, the fibres of the leaves of

I think it more than probable that the Society of Arts, &c. do not possess any specimen of the vegetable fibres, commonly called Ejoo in the East Indies. I have, therefore, the pleasure of sending you a parcel of that substance, consisting of six small rolls, the produce of a small tree for one year. The tree produces, on an average, six leaves every year, and each leaf yields from about four to twenty ounces. It is No. 4. of my first paper on the comparative strength of various vegetable fibres, published in the xxii. vol. of the Society's Transactions. A description of the tree has lately been published by Labillardiere, under the name of *Arenga Saccharifera*. It is the *Anou* of Marsden in his History of Sumatra, page 77: in Rumphius's Herbarium Amboynense, vol. i. page 57, and table 13. a very full account of this valuable palm will be found. By Loureiro, in his Flora Cochinchinensis, p. 759, it is called Borasus Gomutus. The cultivation of this beautiful, stately, and very useful palm may, I think, with the prospect of great advantage, be encouraged in the West Indies. For, besides the above-mentioned fibres, which are in high estimation for thick cordage and cables in India, this palm furnishes sugar, and abounds, as before mentioned, probably more than any other, in wine, which, in its recent state, is a pleasant and wholesome beverage, and is also converted by the Malays into ardent spirits; and when the tree arrives at maturity, the pith of it is one of the varieties of sago meal used by these people in their diet.

the Arenga Saccharifera.

Made into ropes.

Affords much wine and sugar.

A substitute for cork.

I have the pleasure also of sending you a specimen of a most curious, light, vegetable, substance, the spreading stems of *Aeshynomene Aspera*, a water plant, called by the Hindoos and Bengalese *Solah*, and *Fool-Solah*. It is employed by them for a variety of purposes, such as floats for fishing nets, artificial flowers, &c. Might it not be advantageously employed instead of cork, in making jackets to swim with, and in life-boats, &c.? At all events, the bare circumstance of making known the existence of such a

plant,

plant, and the place in which it flourishes, will, I am persuaded, be acceptable information to the Society.

I am, Dear Sir,

Your most obedient humble Servant,

W. ROXBURGH.

III.

Extract from a Dissertation on Coffee, its History, Properties, and the Mode of obtaining from it the most pleasant, wholesome, and economical Beverage: by ANTONY ALEXIS CADET DE VAUX, *Member of various Academies: with its Analysis, by* CHARLES LEWIS CADET, *Apothecary in ordinary to his Majesty the Emperor, Professor of Chemistry, &c.**

PASSING over the historical part, which is sufficiently known, we shall confine ourselves to the chemical and economical.

Raw Coffee treated with Water.

When boiling water is poured on coffee as we find it in the shops, it acquires a yellowish green colour.† If the action of heat be continued, the decoction grows brown, and a light scum rises, which remains insoluble. The decoction passes clear through the filter, but becomes turbid on cooling. A little caustic potash poured into this decoction gives it a deeper brown, and ammonia produces a similar effect. Lime water forms in it a copious flocculent precipitate. Sulphate of iron converts it into a black ink. Solution of gelatine is not rendered turbid by it. Oxigen-

Decoction of raw coffee treated with reagents.

* Journal de Physique, Vol. LXIII. p. 216, Sept. 1806.

† When coffee is fresh gathered, its decoction is of a fine emerald green. A lake might be made of it, and Mr. Dupont de Nemours informs me, that in the West Indies it is used for washing and colouring maps.

Lake from coffee.

ized

ized muriatic acid deprives the decoction of its colour but in part; and, if an alkali be added to this mixture, it becomes red.

Distillation.

Distilled yielded a volatile oil.

I distilled eight pounds of water with a pound of raw coffee, and obtained an aromatic water, on the surface of which were a few drops of a concrete oil, similar to that of the *myrica cerifera*, or candleberry myrtle. The decoction remaining in the still was viscous. This I diluted with a little water, and poured into it alcohol. A copious precipitate was thrown down, which, collected on a filter, was soluble in water, and had all the characters of mucilage.

The decoction gummy.

Resin left in the coffee.

The coffee from which the water had been distilled, being dried in a stove, and digested in alcohol, afforded a tincture, which gave a precipitate on adding water.

The decoction turned litmus green,

The aqueous decoction of raw coffee does not redden vegetable blues. With litmus it even produces a green. All the chemists who had analysed coffee before me have said, that the decoction held in suspension a free acid, which reddened blue vegetable colours. Geoffroy even went so far as to assert, that water distilled from coffee by the heat of a water bath was rendered very sour. I have tried five different sorts of coffee, and repeated the experiment more than twenty times, but the decoction never appeared sour to me.

and contained no acid.

Decomposed alum.

It decomposes sulphate of alumine, and precipitates its earth, which it colours slightly.

Raw Coffee treated with Alcohol.

Alcohol extracts its resin,

Alcohol becomes slightly tinged by standing on dry coffee, even without heat, and holds in solution a considerable quantity of extracto-resinous matter. If water be added to this tincture, it turns milky, and the resin falls down of a dirty white colour. With a solution of sulphate of iron the precipitate is green; with muriatic acid it is fawn-coloured.

leaving extract and mucilage.

The coffee exhausted by alcohol, and afterward treated with water, still furnishes extractive matter and mucilage.

Immediate principles of

From these preliminary experiments we may conclude, that raw coffee contains, 1. an aromatic principle soluble in water;

water: 2. a very small quantity of essential oil: 3. a resin in tolerable abundance: 4. a gum in greater quantity: 5. gallic acid, but no tannin: 6. extractive matter: 7. a little albumen. raw coffee.

Observations.

If the decoction filtered while hot become turbid on cooling, it is because it holds in solution by means of the heat a little resin. Alkalis render it brown; their usual effect on vegetable decoctions. Lime water precipitates it, because on the one hand gallate of lime is formed; and on the other the gummy extractive matter unites with the earth, and carries it down. The same may be said of the sulphate of alumine. Spirit of wine separates the mucilage, because gums are not soluble in alcohol; and water precipitates the alcoholic tincture, because resins are insoluble in water. This precipitate by water is white in consequence of its extreme division: by sulphate of iron green, because it is mixed with gallate of iron: by oxigenized muriatic acid fawn-coloured, because the oxigen, attacking the resin, sets bare a little carbon. The insoluble scum formed on the surface of the decoction is a little vegetable albumen coagulated by boiling water. To obtain this, it is necessary, that the water should stand some time on the coffee cold, before it is heated. General remarks on them.

Proportions by Approximation.

Though it is not of much use to inquire into the proportions of the immediate principles of coffee, since these proportions must vary as the berry is more or less ripe, and according to the place from which it comes, and the time it has been kept, I have deemed it not superfluous, to estimate them as nearly as may be. After several comparative experiments I have found, that eight ounces of coffee afford nearly Proportions of its principles.

	oz.	dr.	gr.
Of mucilage - - -	1	0	0
Resin - - - - -	0	1	0
Colouring extractive matter -	0	1	0
Gallic acid - - - -	0	3	36
Parenchyma - - -	5	3	36
Vegetable albumen - - -	0	0	10
	7	1	10

Coffee germinates in boiling water; but not in alcohol.

It has long been known, that coffee germinates in boiling water (see Bomare's Dict. art. *Café*,) and I have verified this fact. But it does not germinate in boiling alcohol; either because the temperature is not sufficiently high, or because the water is necessary to its germination, or because alcohol destroys its vegetative action.

Martinico and Bourbon coffee alike.

I have compared the decoctions and tinctures of three different sorts of coffee; those from Mocha, Martinico, and the Isle of Bourbon. The last two appeared to me to furnish the same principles in the same proportions, but that of

Mocha different.

Mocha differs essentially from the others. Its decoction was much less saturated, its alcoholic tincture was higher coloured; it contained less gum and less gallic acid, but more resin and more aroma.

Torrefaction.

To know what changes are produced in coffee by roasting, I examined the phenomena that take place during its torrefaction in the open air.

Effect of roasting.

At first the coffee, being penetrated by the caloric, increases in bulk: it crackles, and becomes fawn-coloured: the arillus or pellicle that envelops the seed, separates, and as it is very thin and light the least breath blows it away. The coffee then diffuses a very agreeable aromatic smell. This vapour grows more intense; the seed smokes, and turns brown: presently the smell changes, and becomes slightly empyreumatic; the coffee sweats, and becomes oily on its surface*; it ceases to smoke, and if the action of the fire be continued it is carbonized.

Difficult to roast it to a proper point.

The interval that separates the instant that the coffee becomes coloured from that of its carbonization is sufficiently long, to render it difficult to determine the point at which we should stop, in order that the berry may retain its most agreeable properties; but in order to approximate this point,

Fat oil in coffee.

* Mr. Parmentier wrapped up some roasted and sweating coffee in blotting paper. This paper, imbibing the oil, remained greasy and transparent for more than a year, which indicates the existence of a fat oil in the berry. I could not separate any such oil, however, either by pressure, boiling in water, or the action of caustic alkalis.

which

which is of so much importance to be known, I divide the process of roasting into three distinct periods: 1, that in which the berry loses its natural colour, and assumes that of bread raspings, or dried almonds: 2, that in which it acquires the brown red of a dry chesnut: 3, that in which, become almost black, it is still not charred. Three stages of this process.

I took six ounces of Martinico coffee, divided them into three parts, and roasted them separately in these three degrees. Experiment.

The two ounces slightly roasted, and of the colour of dried almonds, lost on the fire two drachms. These I shall call No. 1. Lost of weight $\frac{1}{3}$.

The two ounces roasted to a chesnut colour lost three drachms. These I call No. 2. $\frac{3}{16}$,

The two ounces roasted to blackness lost three drachms forty-eight grains. These I shall distinguish as No. 3. or near $\frac{1}{4}$.

No. 1. passed through the mill with difficulty. Infused cold, the infusion contained tannin, and precipitated solution of gelatine; it was very aromatic,* and had the flavour of almonds; there was not the least bitterness, but a sufficiently decided harshness. Infused hot its aromatic flavour was the same; and its taste reminded me of that of the almond cake called *nougat*. It was not at all bitter, and the harshness was less perceptible. Slightly roasted.

No. 2. was more easy to grind. To cold water it gave out less tannin; its aromatic flavour was weaker, and it had Higher roasted.

* The object of retaining the aroma, which is dissipated by a strong heat, has given rise to two processes, which are not altogether ineffectual. The first, adopted in India and by some persons in France, consists in putting into the cylindrical roaster a little fresh butter, when the coffee begins to be coloured. No more must be used than will slightly varnish the surface of the berries.—The butter retains a part of the essential oil, that would have evaporated. It is not a bad method, but sometimes it imparts to the coffee a peculiar flavour, which every body does not like. Two methods used to retain the aroma: butter in the roasting;

The second consists in spreading the roasted coffee, while yet hot and sweating, on writing paper, and powdering it lightly with sugar. The sugar absorbs the oil of the coffee, and retains its aroma; but it does not appear to me to increase the pleasantness of the coffee, and renders us uncertain how much sugar to put into a cup. powdering with sugar after it.

more

more of the taste of burnt sugar, but neither bitterness nor harshness. Infused in hot water it gave out neither more taste, nor more aroma.

Very highly roasted.

No. 3. was reduced to powder very easily. To cold water it imparted scarcely any aroma: its taste was empyreumatic, and slightly bitter: and the precipitate it afforded with solution of gelatine was hardly perceptible. The infusion in hot water was more bitter, more empyreumatic, and had a more distinct aroma.

Roasting increased the gum and acid, diminished the tannin.

All these infusions contained mucilage and gallic acid, but in an inverse ratio to the tannin; for the proportions of gum and acid increased with the degree of torrefaction, while the tannin diminished.

Gallic acid a modification of tannin.

Mr. Bouillon Lagrange, in an excellent paper on galls,* had already considered the gallic acid as a modification of tannin; and these experiments tend to confirm his opinion.

Examination of the Roasted Coffee.

As the immediate principles of coffee are not equally soluble or volatile, it was necessary to make a comparative examination of the hot and cold infusions of the three sorts of coffee, as well as of their decoctions.

Infusion in Cold Water.

Roasted coffee macerated in water,

I poured eight ounces of distilled water on one ounce of roasted and ground coffee, and after they had stood together two hours, I filtered the liquor. The infusion was of a very clear brown, did not redden blue paper, was blackened by sulphate of iron, and slightly precipitated solution of gelatine. Alcohol separated from it a little mucilage, and gave the infusion the smell of juniper. Mocha, Bourbon, and Martinico coffee exhibited the same characters.

Hot Infusion.

Infused,

I infused an ounce of roasted and ground coffee for a quarter of an hour in eight ounces of water at 70° (190° F.) This infusion did not redden litmus, or precipitate solution of gelatine, but formed ink with sulphate of iron. Alco-

* See Journal, p. 58 of the present Volume. T.

hol

ol separated more gum from it than from the cold infu-
ion. The three sorts of coffee comported themselves the
ame in these experiments.

Decoction.

I boiled two ounces of powdered coffee in one pound boiled.
f water, and continued the boiling for two hours. The
mell of the decoction was infinitely less agreeable and aro-
natic than that of the infusion. It did not change the co-
our of blue paper, or precipitate the solution of gelatine,
ut was blackened by sulphate of iron. Alcohol separated
rom it much more mucilage than was found in the infusions,
n proportion to the quantity of coffee. The three sorts of
coffee afforded the same results.

If a filtered and limpid decoction of coffee be boiled a Effect of long
ong time exposed to the air, it grows turbid, and deposits boiling.
a black powder, which has sometimes been mistaken for
resin, but is only extractive matter highly oxigenized. Phy-
sicians and apothecaries have not yet sufficiently examined
the action of the air on vegetable decoctions; but they might
derive from it some information respecting the more or less
active properties of certain medicines.

Extract of Coffee.

The decoction of coffee, when filtered and evaporated Extract.
to the consistence of an extract, has no longer the aromatic
odour of the infusion. Its taste is bitter. Heated with
alcohol, this extract colours it with its extractive matter,
but the tincture affords no precipitate on the addition of
water. Hence we may conclude, that the decoction of
coffee, after it has been filtered or stood to settle, contains
no resin.

Spirituous Tincture of roasted Coffee.

Roasted coffee digested in alcohol affords a high coloured Tincture.
tincture, from which water precipitates a larger quantity More resin in roasted than raw coffee.
of resin, than from the tincture of dry or raw coffee.—
From the latter the resin is white: from the tincture of
roasted coffee it is fawn coloured.

Observations

Observations.

Effects of roasting.

It follows from these experiments, that roasting develops the odorant and resinous principles of coffee, and forms in it tannin, which is soluble only in cold water. This is a very singular phenomenon. Gallic acid manifests itself in coffee, at every temperature of the water employed as a menstruum. The gum and colouring extractive matter are more abundant in the decoction than in the infusions; but the aromatic principle is more perceptible and more agreeable in the latter.

Distilled Water of roasted Coffee.

Water distilled from roasted coffee.

I distilled several quarts of water from roasted coffee. The water was impregnated with the aroma of the coffee, and carried over with it some atoms of concrete essential oil, like that obtained from the distillation of raw coffee. Reagents did not demonstrate the presence of any substance in solution in this water.

Infusions and Decoctions compared.

Treated first with cold water

To find the different solubility of the principles of coffee, it remained for me to subject the same powder of roasted coffee to the successive action of infusion and decoction. For this purpose I placed in a filter two ounces of coffee, and passed cold water through it, till the reagents ceased to indicate the presence of the matters in solution. Sixty-eight ounces of cold water were necessary to divest the coffee of all the matter thus soluble. I divided this water into seventeen portions of four ounces each, as they passed through the filter. All these contained gallic acid in proportion to the order in which they passed through: the first four took up gum; but only the first indicated the presence of tannin, by precipitating a solution of glue.

then with hot,

The coffee having been taken out of the filtre, and dried on a stove, I poured on it eight ounces of water at 75° (201° F.) The smell of this secondary infusion was pleasant, but weaker than that of coffee prepared for the table. Examined by reagents it furnished a little mucilage, and a great deal of gallic acid; but I found in it neither tannin nor resin.

I took

I took this same coffee, already washed with cold water, and infused in hot, and boiled it in six ounces of water, till they were reduced to four. This decoction contained a great deal of gum and gallic acid, but little aroma, and afforded no trace of tannin or resin with reagents. and lastly boiled.

Observations.

These experiments prove, that cold water divests roasted coffee of the little tannin it contains, of part of its extractive matter, and of great part of its aroma; but that it takes up only a small portion of its gallic acid, and of its gum. We perceive, that the hot infusion is more loaded with both of the latter principles; but that its aroma is weaker. Lastly we find, that long boiling dissipates in a great degree its odour, but is highly loaded with gum and gallic acid. If it be found to contain resin, this is only suspended in it, disturbs the transparency of the liquor, and is deposited by standing. Effects of cold water. of hot. of boiling.

Ashes of Coffee.

Though it is of little importance to know what coffee reduced to ashes contains, I incinerated about half a pound. The ashes were pretty light. Lixiviated with distilled water, their analysis afforded nothing but a little lime, and a very little potash. I acidulated the lixivium with a small quantity of nitric acid, and the filtered solution precipitated prussiate of potash of a fine blue. Oxalic acid gave with it a copious precipitate. It was not altered by barytes. Nitrate of silver turned it white. Coffee ashes then are composed of carbon, iron, lime, and muriate of potash. I did not think it necessary to ascertain their proportions. Incinerated, and the ashes examined, contain carbon, iron, lime, and muriate of potash.

I had intended here to have concluded my analysis, when Mr. Parmentier read at the Society of Pharmacy a very copious memoir on coffee, written by Mr. Payssé, an apothecary, who has already published several very interesting works. It is said in this memoir, 1st. that the precipitate formed by the mixture of the decoction of coffee* Coffee analysed by Mr. Payssé.

* This is a mistake. It was the precipitate formed by the acid of coffee, as Mr. Payssé calls it, obtained in the way in which Mr. Chenevix found what he considers as a distinct principle, by precipitating the decoction with muriate of tin, and separating the tin by sulphuretted hidrogen. T.

with sulphate of iron, is soluble only in the nitric, sulphuric, phosphoric, and oxalic acids: 2d. that coffee contains no gallic acid: 3d. that it contains a peculiar acid, *sui generis*, which the author calls *coffic* acid, and which he obtained by following the process of Mr. Chenevix, that is, making a decoction of raw coffee, filtering, precipitating by muriate of tin, and decomposing this precipitate by sulphuretted hidrogen gas *.

Contains a peculiar acid.

The authority of the name of Chenevix, and the accuracy with which the processes of Mr. Payssé are generally conducted, induced me to make several experiments, in order to confirm the new facts that were announced.

Decoction.

I boiled two ounces of Bourbon coffee in a pint of water, for two hours. The decoction exhibited the same phenomena as I had already observed. It assumed a yellowish green colour, which became more bright by the separation of the little albumen, and let fall a precipitate of oxigenized extractive matter. This decoction, when filtered, turned the aqueous tincture of litmus green.

Precipitated by sulphate of iron,

I mixed a portion of this decoction with a solution of sulphate of iron, and obtained a precipitate of a very deep blue, inclining to black. This precipitate I redissolved in oxigenized muriatic acid, strong and weak acetic acid, tartarous, citric, and even benzoic acid.

and by muriatic acid.

Muriatic acid rendered the liquor yellow; but it resumed its transparency, after letting fall a tolerably heavy precipitate of oxigenized extractive matter. This precipitate, being redissolved by ammonia, gave a fine brown red colour to the liquor.

Precipitate by sulphate of iron,

The immediate precipitate of the sulphate of iron dissolved by acetic acid, comported itself nearly in the same manner, except with regard to the colour, which was of a violet blue. It was likewise redissolved by ammonia. The

and by acids.

other acids afforded nearly the same precipitate as the mu-

* Mr. Chenevix does not say, that the substance he obtained by this process was an acid, but a new product, the nature of which he does not determine. [See Journal, Vol. II. p. 114.]

riatic;

riatic; their action in general being in the ratio of their acidity.

I treated a precipitate of sulphate of iron, obtained by means of gallic acid, in the same manner, and the results were no way different from the preceding. The precipitate of iron was a gallat.

The remainder of the decoction of coffee I precipitated by muriate of tin. This salt occasioned a very copious sediment in the liquor, which I washed with water, till no marks of acidity were perceptible in it. I afterward put this metallic compound into a tubulated phial, and poured on it a considerable quantity of distilled water. I then adapted the phial to a Woulfe's apparatus, so as to pass sulphuretted hidrogen gas over the precipitate. The first portions of gas changed the mixture brown, and this colour grew deeper and deeper, in proportion as the liquor became saturated with the gas. The precipitate was decomposed: a hidrosulphuret of tin was formed: and the disengaged acid was taken up by the liquor. The liquor, being first filtered, was evaporated by a gentle heat, till it was reduced to one eighth. This product, considered by Mr. Payssé as coffic acid, appeared to me to be nothing but gallic acid. I not only subjected it to the action of all the reagents comparatively with acid obtained from galls in the usual way; but, that I might leave no doubt on this head, I treated galls by the same process. In this decoction the muriate of tin formed a more abundant precipitate than in the decoction of coffee: the precipitate, decomposed like the preceding by sulphuretted hidrogen gas, afforded me an acid of the same colour, the same taste, possessing the same properties, and differing only in quantity. I think, therefore, I may conclude, that the *coffic* acid does not exist; but that coffee contains less gallic acid than nut-galls.

Precipitated by muriate of tin, and the tin thrown down by sulphuretted hidrogen.

Not a peculiar acid, but the gallic. Proofs.

It is possible, that this gallic acid may exhibit in its combinations and compounds some slight shades of difference from the acid obtained from the gall of the oak, but it is nevertheless of the same nature.

May have some shades of difference when obtained from galls,

The immediate materials of vegetables, as is well known, though of the same kind, and perfectly analogous, are not strictly identical. The gums and sugars exhibit differences in

as is the case with other immediate principles of vegetables.

in their physical properties; yet all mucilage, all saccharine matter, is the same chemically considered*. Proust has demonstrated, that tannin obtained from different vegetables displayed some differences: it is possible, therefore, that gallic acid obtained from coffee may not be absolutely the same as that from galls, but it is not a distinct acid.

Recapitulation.

Principles of coffee.

It appears to be demonstrated from the analysis above given, that the coffee berry contains abundance of mucilage, a great deal of gallic acid, a resin, a concrete essential oil, albumen, and a volatile aromatic principle. To these principles are added those found in many vegetables; namely, lime, potash, iron †, carbon, &c. Torrefaction develops the soluble principles; but it must be moderate, if we would retain the aroma, and not decompose the acid, the gum, and the resin.

Effects of roasting.

Tannin produced by it.

The roasting adds a new principle, which is tannin, though in very small quantity. The cold infusion is very aromatic; but it contains little mucilage or gallic acid. The hot infusion retains some of the aroma; and the principles dissolved in it are in such proportions, as to be agreeable to the taste. The decoction has little aroma, and is much loaded with gum and gallic acid, the resin too may even be suspended in it, and it is less pleasant to the taste than the infusion.

Treated with water.

Different sorts of coffee.

The coffees from the island of Bourbon and Martinico

* The fecula of potatoes does not resemble that of wheat, and this again differs from the fecula of cassada, sago, salep, arum, maize, &c. Yet chemists would say of all these, that it is an amylaceous substance, and find in them the same leading characters.

Iron in coffee with gallic acid, yet not indicated by colour.

† The presence of iron in vegetables is very common; but that of iron in a vegetable containing a great deal of gallic acid, without this acid being combined with it, and imparting a blue or black colour to the vegetable, is a very remarkable phenomenon. It appeared to me deserving of inquiry, and I made a comparative analysis of the ashes of galls, in which also I found a sensible quantity of iron. [It may be observed, however, that galls have very often an evident blue tinge; so much so, that it is commonly considered as an evidence of superior quality. T.]

do

do not perceptibly differ from each other; but that from Mocha, as was observed above, is more aromatic, less gummy, and more resinous. It is probable, that the resin of coffee, as that of most astringent vegetables, has peculiar medicinal properties. As it is obtainable neither by infusion nor decoction in water, the habitual use of coffee can afford us no insight into its action on the animal economy. It is for physicians to make such experiments on the subject, as they may deem useful.

Perhaps the resin medicinal.

If I might be allowed from this analysis to draw precepts applicable to the domestic use of coffee, I would say, that it is possible to make excellent coffee from every kind of the berry found in the shops, provided it be not damaged. Amateurs look to three points of perfection in coffee: they would have in it an agreeable aroma, a slightly rough taste, and a certain density, which is called *body**. All these objects, I believe, may be obtained, by proceeding as follows.

Domestic use of coffee.

1. Choose a coffee, that, when dry, has no taste of mouldiness, or is not damaged by salt water.

How it is to be made in the greatest perfection.

2. Divide the quantity to be roasted into two equal parts.

3. Roast one portion only till it is of the colour of dry almonds, or bread raspings, and has lost one eighth of its weight.

4. Roast the other till it is of a brown chesnut colour, and has lost nearly one fifth of its weight.

5. Mix both these together, and then grind them.

6. Let the coffee be both roasted and infused the day on which it is to be drunk.

7. Pour four cups of cold water on four measures, or two ounces of coffee, and when the water has run off, set it by.

8. On the same coffee pour three cups of boiling water, and mix the water that runs off with the preceding. You should thus have six cups of coffee.

* Some of the eastern nations value this density so highly, that they reduce their coffee to a very fine powder, leave the grounds in the infusion, and drink it as thick as a kind of thin pap.

9. The

9. The moment you are going to drink the coffee, heat it over a brisk fire, but do not let it boil.

10. The infusions should be made in a china, earthenware, or silver pot.

Such is the process pointed out by theory, and I can recommend it from experience.

Payssé's analysis of raw coffee.

To give the whole of Mr. Payssé's memoir, alluded to above, would occupy too much room; but we apprehend it will be acceptable to the reader, to have subjoined the conclusions which that gentleman draws from his chemical investigation of raw coffee; particularly as he differs, in some respects, both from Mr. Cadet, and from Mr. Chenevix.

It contains a peculiar acid,

1. It results from all these experiments*, that coffee contains a peculiar acid sufficiently characterised: that it is in some respects free, since the powder of the berry speedily reddens blue vegetable tinctures: and that cold water, or even alcohol, can separate it in a state more or less pure.

which decomposes most metallic solutions.

2. That the acid decoction of coffee easily decomposes most of the metallic solutions, as those of tin, lead, iron, &c.

Precipitates of these by the decoction more copious, because less pure.

3. That the precipitates obtained by a mixture of this decoction with the metallic solutions are more copious than those formed by the pure acid, because the decoction contains extractive matter, colouring matter, albumen, &c. beside the acid. For the colouring matter is partly precipitated by the affinity it has for the compound, formed of the coffic acid with the metallic base; and on the other hand the albumen, being separated from the acid which promoted its solution in the liquid, falls down and increases the bulk of the precipitate. To be convinced of the truth of this, nothing more is necessary, than to boil a coffat of tin, lead, or alumine, in a coloured vegetable decoction, to obtain the result in question.

Methods of obtaining the acid.

4. That the acid of coffee may be obtained sufficiently pure by mixing a decoction of coffee in water, or a tincture of it in alcohol, with the muriate of tin or of lead, and afterward decomposing this combination by sulphuretted

* *Annales de Chimie*, Vol. LIX. p. 196. August, 1806,

hidrogen,

hidrogen, as Mr. Chenevix did; or by decomposing coffat of lead by the sulphuric acid.

5. That this new acid is not crystallizable in the state in which I obtained it; but is completely soluble both in water and in alcohol. *It is soluble in water and in alcohol.*

6. That it is capable of decomposing the prussiate of iron contained in the prussiate of potash, forming with this metal a green precipitate: and in this respect it may be of great service to chemists for obtaining prussiate of potash perfectly pure, which hitherto they have been unable to deprive of a certain portion of iron, it retaining this with so much obstinacy. *Decomposes prussiate of iron, and forms a green precipitate.*

7. That the colour it communicates to the oxigenized and green sulphate of iron appears altogether new *. *Its effects on sulphate of iron.*

8. That the attraction of the compounds it forms with tin, lead, antimony, and alumine, for the colouring part of vegetable decoctions or infusions, may render it of use in the art of dyeing. *May be of use as a mordant.*

9. That the different kinds of coffee contain it in nearly the same proportion; and that it exists without alteration, though in smaller quantity, in the infusions and decoctions of coffee roasted in different degrees, as well as in the products of its distillation. *In all kinds of coffee, raw or roasted.*

10. That the comparison I made of the properties of this acid with those of the gallic acid and tannin did not show me any identity of nature between these three very different substances. *Differs both from gallic acid and tannin.*

11. That the peculiar principle obtained by Mr. Chenevix was, no doubt, the acid substance in question; though it was not examined with sufficient strictness by that learned chemist. *It was the peculiar principle obtained by Chenevix.*

12. That, having examined the infusions and decoctions of different sorts of roasted coffee, they did not afford me any proof of the existence of tannin, by mixing them with gelatine, as Mr. Chenevix asserts. *No tannin in roasted coffee.*

* Coffic acid, dissolved in six times its weight of water, added to a solution of oxigenized sulphate of iron, immediately gave it a fine green colour; and after it had stood six hours, a precipitate of the same colour fell down. To a solution of green sulphate it gave at first a very light green tinge, but this grew deeper, after it had been some time exposed to the air.

13. That

Coffic acid unites with various bases,

13. That the acid of coffee is capable of uniting with a great many bases, and forming peculiar salts, decomposable with more or less facility by fire, and the powerful acids; and that its affinities appear to follow a law altogether different from that of most of the known acids, since its union with alkalis seems to be the weakest.

but most feebly with alkalies. Decomposed and reduced to malic acid.

14. That it is decomposed by hot sulphuric acid, and the nitric, muriatic, and oxigenized muriatic acid; and reduced by the latter, as well as by the nitric, to malic acid.

Contains much carbon with little hidrogen.

15. That, from the products obtained by its analysis by fire, it appears to be composed of a great deal of carbon, with less hidrogen and oxigen.

Component parts of the watery extract.

16. That 100 parts of aqueous extract of coffee, the product of about 750 parts of the berries, afforded me of coffic acid 55, extractive matter 25, vegetable albumen 5, and resinous matter 9; the loss being 6.

17. That, to adopt the language of modern chemistry, this acid ought to be called the *coffic*, from the name of the substance from which it is taken.

Ashes.

18. That the incinerated residuum of coffee is composed of muriate of potash, lime, and a portion of iron, the quantity of which was too small to be ascertained.

Remote principles of coffee.

19. Finally that coffee, from all that has been said, is a substance containing carbon in much larger proportions than hidrogen, oxigen, or azote; the existence of all these having been evidently demonstrated by the formation of oil, pyromucous acid, carbonic acid, and ammonia united with this acid, in the destructive distillation of coffee.

IV.

Account of the Existence of Platina in the Silver Mines of Guadalcanal, in the Province of Estremadura. By M. Vauquelin*.

Platina found only in South America.

HITHERTO platina had been found only among the gold mines in South America, at Santa Fe, and in the bailiwick of Choco. There was a report a few years ago,

* An. de Chim. Vol. LX. p. 317, Dec. 1806.

that

that platina had been discovered in Siberia; but this has no more been confirmed, than that spread fifteen years ago, of its existence in a ferruginous sand at St. Domingo.

Old mines in Estremadura, lately reopened, furnish platina,

Having been lately employed to analyse the ores of the celebrated mines of Guadalcanal, in Estremadura; which, after having been shut up for a long time, have lately been opened again at a fresh place; I discovered in one variety of these ores the presence of a large quantity of platina. This ore is of a gray colour, and bears considerable resemblance to that known in France by the name of gray silver ore, the *fahlerz* of the Germans, [properly gray copper ore.] It contains copper, lead, antimony, iron, sulphur, silver, and sometimes arsenic. Its gangue most commonly consists of carbonate of lime, to which are added sulphate of barytes and quartz. In the month of October last, I communicated this discovery to my learned colleague, Mr. Fourcroy, whose knowledge and friendship have been continually serviceable to me for these twenty years. This fact, which appeared to him highly important, he persuaded me to verify, by experiments so numerous and varied, that they should be open to no dispute. I followed his advice; and the following are the results of my researches, which have left no doubt in my own mind, though hitherto I have been able to operate on no considerable quantities of ore.

from a variety of gray copper ore.

It is in various proportions, from 10 per cent. to almost nothing.

The platina appears to exist in various proportions in the silver ores of Guadalcanal. Some specimens afforded me as much as twenty marks to the hundred pounds, or ten per cent; and some exhibited merely traces of it, that were scarcely perceptible; which indicates, that it does not form an essential, or properly constituent part of the ore, and that it is simply mixed in irregular quantities in various parts of the vein. The silver appears to be in the same case. In fact this varies greatly in its proportions, as I have found in the gray ore of Guadalcanal from four marks to fourteen, or from two to seven per cent of the whole weight.

The silver from two to seven per cent.

Mode in which it was extracted.

The process I employed to extract the platina from these ores, after several comparative trials, consists in the following operations. 1. After having reduced the ore to a fine

fine powder, I roasted it with a gentle heat, stirring it constantly, to avoid the fumes. 2. I then fused it with an equal quantity of common potash, and thus obtained a metallic button, consisting of platina, silver, lead, copper, and sometimes a little antimony. The iron and part of the lead remained in the scoriæ. 3. I then separated the copper, antimony, and remainder of the lead, by cupellation; which left me only the silver and platina. 4. I parted the platina from the silver by means of aqua fortis, or the nitric acid of the shops, which dissolved the silver, and left the platina behind. This I washed, and heated again, to give it the metallic lustre. 5. If the lead naturally found in the first metallic button were not sufficient, to carry off all the copper in the process of cupellation, I subjected the metal to this operation a second time with a fresh portion of lead. 6. On the contrary, if the quantity of silver were too small to allow the aqua fortis to act on the alloy, I added a fresh portion of this metal, as in parting it from gold. 7. I ought to caution the reader, that the aqua fortis, if it be not sufficiently diluted, will dissolve a portion of platina at the same time with the silver; which is easily perceived by the brown colour the solution assumes.

Parting necessary to obtain the platina, and even the silver.

If platina be found in the gray ore of Guadalcanal in a proportion that will allow it to be extracted with advantage, of which, according to my first researches, there can scarce be any doubt, it will require to be parted by means of aqua fortis, in the same manner as is practised with respect to the gold extracted from silver ores: and even if there be no advantage to be derived from the platina extracted by this process, it will be necessary to employ it to obtain the silver; for by any other mode these two metals will be found united together from the similarity of their properties.

It is in the metallic state.

Platina appears to exist in the metallic state in these ores, for the simple acids do not dissolve the smallest quantity of it, and it is constantly found among the sulphur and silex, when the latter constitutes part of the gangue. It was indeed by examining these residuums of the ores, and treating them in succession with nitric and muriatic acid, that I first perceived the platina.

What

What is remarkable on the present occasion is, that neither of the four metals recently discovered, which accompany platina in the ore from Peru, is found in that of Spain. This is a consideration of much importance, since it will greatly influence the means of extracting this metal, and since it gives hopes of obtaining it in a state of purity, which cannot be attained with the platina of Peru, but by means of difficult processes and great expense.

Neither of the four new metals found with it.

Will afford pure platina.

If these hopes be realized, as every thing tends to persuade us, we shall have in Europe, and at hand, a precious metal, which will soon become of great utility for the purposes of natural philosophy, chemistry, the arts, and even domestic economy, in fabricating a variety of instruments, vessels, and utensils of every kind; since, with all the advantages that gold enjoys, it unites several properties, that render it greatly superior to gold*.

V.

Carbonization of Turf, or Process by which all possible Advantage may be derived from Products hitherto neglected in that Operation, executed in the Year of the Republic 11; *by* ANTONY THILLAYE-PLATEL, *House Apothecary at the Hotel-Dieu at Paris* †.

THE idea of the experiments, of which I am going to give an account, was suggested to me by the discovery of thermolamps.

* Perhaps this discovery of Mr. Vauquelin may account for the two ancient candlesticks in the cathedral of Hildesheim, in Lower Saxony; made we believe long before any platina could be brought from South America, though we do not know their exact date; and mentioned by Professor Cramer, of that place, in his Letters on Natural Philosophy. These are described as white, and nearly as heavy as gold, and probably therefore consist of such an alloy as would be obtained from a portion of the ore of Guadalcanal, rich in platina and poor in silver; and which Bishop Bernward, their maker, though one of the most skilful metallurgists of his time, did not know how to separate. T.

Ancient candlesticks probably from the same, or a similar ore.

† Annales de Chimie, Vol. LVIII. p. 128, May, 1806.

I was

Manufactory for charred turf.

I was at Rouen, employed at the hospital under Mr. Robert, chief apothecary, a man for whose talents I have the highest respect, on more accounts than one; and he was repeating some experiments relative to these discoveries, which when I saw, I conceived the idea, that the apparatus might be employed for more carbonizing processes than one; and I communicated to him my thoughts respecting some questions, that had been put to me, on the possibility of converting turf into a charcoal, capable of being substituted for that wood.

It had been proposed to me, to form an establishment capable of manufacturing a very large quantity at a time. Mr. Robert approved my scheme; and, assisted by his judicious advice, I undertook a manufactory of turf charcoal some miles from Gournay.

I had already obtained some success, when circumstances foreign to the business occasioned it to fall to the ground, and ruined an undertaking on which I had long rested all my hopes.

Though I here bring forward new methods, it does not follow, that the product of some manufactories, among others those of Meaux near Paris, are not of good quality, as the public begin to be sensible.

Turf first pressed.

In order that the turf may present the greatest possible substance in a given bulk, I expose it to a regular continued pressure; by which means it quickly loses all the water it contains, its desiccation in the air is more speedy, and thus we gain the advantage of a saving of time.

Mode of placing it to be charred.

After this pressure, though the charring might be performed without this preliminary operation, care is taken to place the turfs so, that the masses they form shall present demiobstructions to the air, to accelerate its currents.

In this state it is subjected to carbonization by means of an apparatus, which will be described below.

Products by distillation.

Observation having proved, that vegetable substances afford advantages even in their distilled products, I concluded, that the oily and condensable matters should be separated from the gasses, which I intended to employ as a supplementary support of the combustion.

The gasses supply fuel.

This observation is so much the more valuable, as these very

very gases may supply the place of one fourth of the turf or combustible employed in the carbonizing fire, an advantage hitherto neglected.

I would beg leave here to remind the reader of some facts relative to the order in which the gasses are disengaged, during the action of caloric applied to vegetable substances.

Effect of caloric.

It is known that caloric, in contact with these vegetable substances, disorganizes them wholly or in part; that it solicits their three remote principles to combine according to their various affinities, and at different temperatures; and that the results are products very different from the original compound.

First products, water, oil, and acid;

Thus, for example, the most volatile substances, or those the principles of which have the strongest attraction for each other at a low temperature, are first disengaged: the water, oil, and vegetable acid, pass over first, whether they were partly contained in the vegetable substance, or that their principles were induced to combine by the predisposing affinity of caloric: but at a high temperature, at a red heat, carbon decomposes water, this ceases to be formed, and the carbonic acid passes over, with carburetted hidrogen surcharged with carbon, and oxidule of carbon; the fixed substances remain in the distilling apparatus; and, if azote be contained in these substances, it is at this period the carbonate of ammonia is disengaged.

then carbonic acid, and carburetted hidrogen.

Carbonization of large quantities different.

Though the phenomena take place in this manner, in small masses, heated equally in all their parts, it is not the case with several hundred weight of materials, the outside of which will be carbonized, while the centre of the mass has scarcely experienced the effect of the caloric acting in the inverse ratio of the square of the distance.

Proportions of the products gradually differ.

Accordingly we may expect to find the products differing in their proportions at different periods: then the water, oil, and acid, will predominate at first, and will subsequently decrease in their proportions, and be more carbonized.

Empyreumatic oil.

We shall then find a black, oily, acrid substance appear, more or less heavy, which is the empyreumatic oil, and indicates a carbonization approaching its end in a favourable manner.

The

Charcoal.

The last result is a fixed black substance, tolerably homogeneous, and weighing more than an equal bulk of charred wood. Frequently, in consequence of the sand, the oxide of iron, and the compactness acquired by the previous compression, this substance is the true charcoal of turf; which sometimes, before it is obtained, furnishes a certain quantity of sulphurous acid, arising from the combustion of the sulphur and sulphate of iron contained in such turf as I have dug. This justifies to a certain degree the complaints of persons, who refuse to make use of this combustible; but this slight defect may be removed by very easy means, which I employ in burning turf in rooms, and of which I shall give the particulars hereafter.

I shall now give a description of my apparatus, which I shall divide into two parts; the first describing the furnace, the second the interior part, which I call *thermolampic*.

The furnace described.

The furnace is square, terminating above in an arch; and in the front appear three apertures, one over another.

The ash-hole.

Its inferior part, in which is the ash-hole, is shaped interiorly like a wedge, the base of which is the aperture, one of the square sides lying uppermost and horizontally, the other, an inclined plane, forming the bottom. This form prevents any ashes from lodging in it to obstruct the fire, and renders the current of air more rapid. It is obvious that the upper part of this ash-hole is formed by the grate, which consists of movable bars of iron arranged parallel to each other by means of a cross piece. This arrangement facilitates the arrival of the air, and accelerates the combustion. Above the ash-hole is the second opening, which is that of the fire-place, and is carefully closed with a large stone shod with iron, and furnished with two rings, to admit a hook, by which the door is removed whenever fuel is to be thrown in. See Fig. 2. Plate III.

The fire-place.

Chimney.

The third aperture is perceptible only by the projecting part of the bottom of the chimney, which suggests that the smoke is obliged to surround the interior apparatus: and this is in fact the case, since it returns to find an exit exactly on a level with the bottom of the thermolamp, which is supported by the interior and anterior part of the furnace.

By

By the chimney it may be observed, that the superior aperture is less than the inferior, which is indispensable in this construction; and this leads me to say, that there always exists a direct ratio between the apertures of the fire-place, the ash-hole, the place where the smoke enters into the chimney, and that at which it finds its exit, which should always be proportional to the height. Thus ash-holes of a moderate depth and aperture, fire-places narrow and well closed, turns (*chicanes*) artfully managed, and a convenient issue for the smoke, all united constitute a furnace, the good qualities of which are demonstrated by experience. See Fig. 1.

Thermolamp.

The second part of my apparatus, which I call thermolampic, is so arranged, that its lower part is horizontal, and forms a long square. The upper part of its whole length is an elliptical arch, terminated at each extremity by a vertical plane, in the middle of which is a funnel or tube bent at a right angle, serving to convey the products that arise by distillation into the middle of a condenser, consisting either of a stone hollowed out, and covered by a plank well luted to it, of a small wooden cask standing upright, or of a cast iron tube surrounded by a stream of water. From these issue tubes to convey the inflammable gasses into the fire-place, that they may serve, as I mentioned above, instead of part of the fuel employed. The funnels above described are furnished each with a key, to intercept the communication between the outer air and the charcoal, while still hot: for experience has proved, that charcoal thus prepared is capable of spontaneous accension. This phenomenon, it is said, may take place in large heaps of charcoal long prepared; and, though I confess I never saw such an accident, it is certainly prudent to guard against it by currents of cold air traversing the heap in various directions. See Fig. 3.

Condenser.

Gas pipes.

Charcoal kindled spontaneously.

The material of which the thermolampic apparatus is composed should be sheet iron, or thin cast iron; though one or more common cylinders may be substituted instead, placed in a suitable manner, and furnished at their extremities with tubes for conveying off the gaseous products, &c.; taking care that one end may be opened by means of

Material.

Opening for the substance to be charred.

a hook, and closed with a proper lute. To all these must be added, that the part of the furnace, through which the substance to be charred is introduced, should be made only of dry bricks, and covered with wet clay.

Position of the apparatus.

Whatever be the form or material of the thermolamp, it ought always to be placed horizontally in the furnace, and have its extremities resting on the side walls. In this situation the bottom and circumference will receive the action of the fire, which must be fed with the bad turf, as I have mentioned, assisted by the gas from the lateral tubes.

Bar for security.

To all this must be added a bar of iron, in the direction of the width of the apparatus, to prevent it from giving way when loaded, and long exposed to heat.

Turf dried by the heat of the smoke.

I have turned the heat arising from the smoke to great advantage, by constructing a kind of stove, to prepare the turf for the charring apparatus. The most suitable means for this drying process, particularly in winter, may readily be conceived.

Sixty pounds of wood charred.

Into an apparatus disposed as I have described, I put upward of sixty pounds of dry wood, that I might form an accurate idea of its advantages; and the following were the results.

Results of the process.

After having commenced the extrication of the gasses with turf of good quality, acquired by pressure, they burned with vehemence, and at the expiration of an hour furnished such a quantity of radiant heat, that the tubes alone, without any addition of fuel, were capable of continuing the operation, affording in the conclusion a perfectly homogeneous charcoal; and I confess, that I never saw what appeared to me a more beautiful sight. How indeed can we behold without admiration a combustible burning itself, and thus saving half the fuel, that would have been required to convert it into charcoal?

Domestic use of pressed turf.

I must not here omit to speak of the use of pressed turf for domestic purposes. For instance, I have found by experience, that turf of a good quality, after having been pressed and well dried, produced heat in the following proportions.

Compared with wood

Five or six parts of turf are equal to four of wood, supposing the fireplace to be extremely accessible to air. There

is

is a great advantage in burning turf therefore, supposing it even not to be pressed, and that in this case it would require two, three, or even four parts to one of wood; for, if we consider the value of both, we shall find, that for the same price three times as much turf as wood may be burned.

Objections to it.

But the public are unwilling to adopt this economical practice, alleging, that turf emits a disagreeable smell *, and gives but little heat.

Not valid in English fire-places.

All these errors would be dissipated, if fire places were constructed nearly like those used in England for burning coal or coak; and I may add, that I have had the pleasure of seeing persons, who were greatly averse to innovation, rejoice at having made trial of a fire place of my construction, for the use of a species of a fuel on which they would scarcely deign to cast an eye. See Pl. IV. figs. 4, 5, 6.

Proportions of charcoal, and of ashes, produced from turf;

Turf does not always afford an equal quantity of charcoal, and of course the quantity of ashes must vary in the same proportions. I have seen turf that afforded 0·38 or 0·40 of charcoal, which left on incineration 0·17 or 0·18 of ashes. I do not mention certain kinds, that have produced 0·50 of ashes, since from these must be subtracted 0·35 of ferruginous sand, which they contained.

and from oak.

The turf that I employed in my manufactory produced, after a well managed process, from 0·38 to 0·42 of charcoal, and yielded from 0·13 to 0·16 of ashes; but I am inclined to think, that, when the saving occasioned by the use of the gases is considered, the quantity of charcoal may be raised by secondary improvements to 0·50. The quantity of ashes produced by turf charcoal will be thought very considerable, when compared with the quantity arising from sound wood. Thus on examining oak freed from its alburnum, and of fifty years growth, we find that fifty parts yield from twenty to twenty-one of charcoal, and from two to two and a half of ashes.

Turf may be used in making bricks, tiles, or earthen ware.

Finally I will add, that pressed turf, or turf of a very good quality, may be used in burning bricks or tiles; and in baking common earthen-ware, &c. for three fourths of

* This smell is produced only by unprepared turf.

the process, finishing it with one fourth of well burning wood; and this earthen ware will be equal to what is commonly made for domestic purposes, as I have found by experience.

To obtain such results, I have myself constructed a furnace on the principles laid down above. Neither was the preparation of the earth for each kind of pottery forgotten: this was the basis of my labour.*

Oily products.

I have already observed, that the oily products received in the condenser might be turned to advantage. These products are frequently divided into two strata: the first water, impregnated with a small quantity of a light oil, and containing acetous or acetic acid: the second a black, heavy, acrid, very penetrating oil, thick like tar, and difficult of solution. This may be used for various purposes.

Acid useful for making iron-liquor, for dyers or calico printers;

The rectification of these oils furnishes a certain quantity of acid, by means of which I have prepared the solution of iron, called in manufactories iron liquor; but it is proper to add a little concentrated acetous acid, in order to prevent the precipitation of the iron in the state of oxide.—With a solution of this sort I have prepared by a particular method patterns of black, on cotton, silk, and woollen, which were not inferior to those dyed in the usual way. I could likewise produce very good nankins; and the buffs on printed calicoes might be prepared with iron dissolved in this acid, thickening the composition with starch or gum, according to the value of the article and the tint required.

What I have said respecting iron dissolved in the acetous acid may be extended to the employment of the empyreumatic oil in dyeing wool, &c. and a number of other processes, too many to enumerate.

and manufacturing verdigris.

Lastly, I am persuaded, that it is possible to oxide copper by means of this acid, which however is afforded in smaller quantity perhaps by turf than by wood.

Improvement in tiles.

* I would beg leave to mention an improvement I proposed in tiles, and which I carried into execution. This consists in making a tongue to them of a triangular shape, the base being very large and at bottom; and with respect to the moulding, it may be sufficient to say, that two men could do the work of four in a given time.

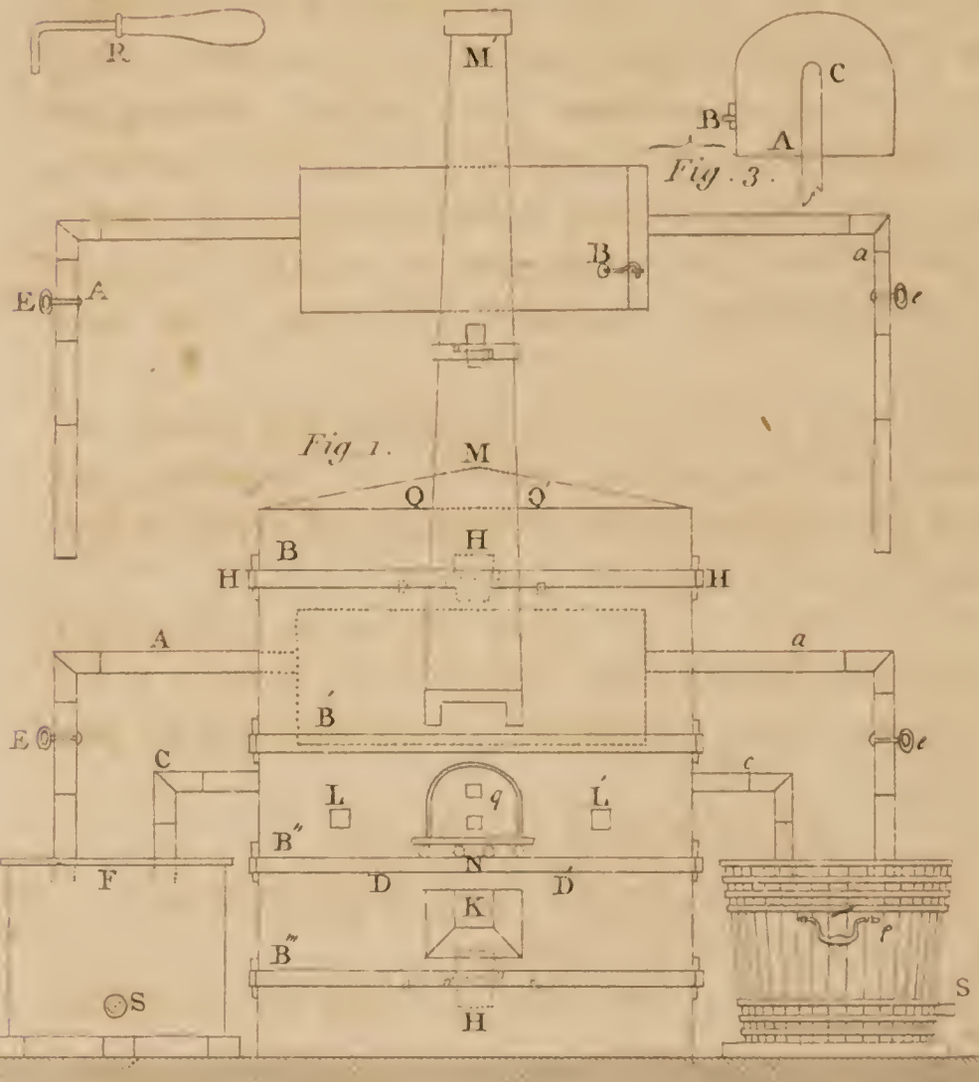
To

Apparatus of M.r Thillaye Platel, for the Carbonisation of Turf.

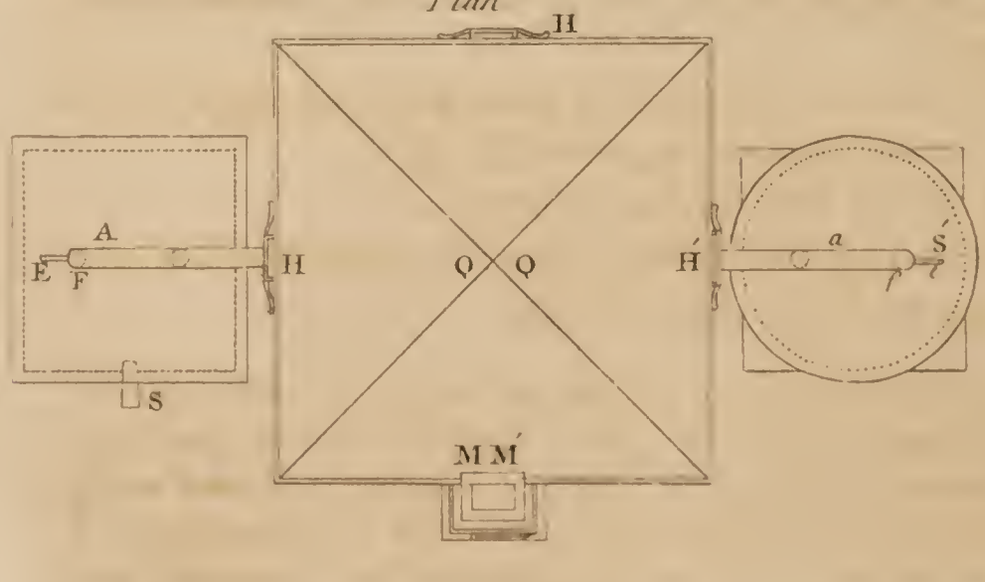
Fig. 2.

Fig. 3.

Fig. 1.



Plan



To produce that preparation of copper, which is called verdigris, the remains of certain cider apples might be used, after the juice has been pressed out, of which there is a great deal throughout Normandy, that is turned to no account, except as a bad kind of fuel. In this case it should be moistened with bad sour cider immediately after it is taken from the press, and put into earthen pots, or little casks, with plates of copper, *stratum super stratum;* washing the oxided copper with our acid, after it has thus stood a month, and then proceeding according to the method of Mr. Chaptal. Various wild fruits, as the sloe for example, might be used in a similar manner, instead of the refuse of the cider press.

The pressed pulp of cider apples applicable to this purpose,

and many wild fruits.

I am indebted to Mr. B. E. Lefébure, my friend and countryman, and a zealous cultivator of chemistry, for the first idea of employing the pressed pulp of apples in preparing the acetate of copper.

The facts related in this paper will prove interesting I hope to science, and of some utility to the public; and if I meet with the reader's indulgence, my wishes will be gratified.*

Explanation of the Figures.

Explanation of Plate III.

Plate III. *Fig.* 1. an interior view of the furnace.

Q, Q′, upper part of the furnace.

M, M′, the chimney, the shape of which may be varied.

H, I′, iron hoops, keyed
B, B′, B″, B‴, other hoops } to strengthen the apparatus.

L, L, little air holes, to accelerate the combustion at pleasure.

q′, the fire-place closed by a stone with two rings, to admit the introduction of the hook R, fig. 2.

N, four iron bars forming the grate.

D D′, an iron bar, fastened into the brick work at each end, upon which the bars N slide.

* The day on which my memoir was received, I was informed by Mr. Vauquelin, that Mr. Lebon, engineer of bridges and highways, inventor of the thermolamps, had applied processes nearly similar to mine to the carbonization of wood in his experiments. I conceive therefore it is but justice to say with Mr. Vauquelin, that nothing can be better contrived, than the apparatus of Mr. Lebon.

K, the ash-hole, nearly in the form of a wedge placed horizontally.

Fig. 2. A, a, tubes to convey the gases from the thermolamp into the condenser.

E, e, keys to cut off the access of the external air to the charcoal, while yet hot.

C, c, tubes to convey the gases that are not condensible into the fire place.

F, f, condensers to be employed at pleasure. F is a stone hollowed out, and properly covered: f, a tub, or, if it be preferred, a small cask.

S, S′, the pipe for the discharge of the distilled fluids.

Fig. 3. The carbonizing or thermolampic apparatus.

A, a, tubes issuing out of the furnace.

B, a crook to keep in its place the part opening at the side, where the masonry forms a door closed with dry bricks, removed and replaced at every operation.

C, body of the apparatus.

The dotted lines express the parts that are concealed, and are continuations of the lines drawn full.

Plate IV. *Plate* IV. *Fig.* 4. Interior view of a fire place for burning turf, or turf charcoal. By this construction it appears, that the disagreeable effects ascribed to the burning of turf are avoided.

A, mantletree of the fire place.

B, a plate of metal, rising and falling in two lateral grooves by means of the two copper buttons, C, C′.

E, the back, formed of a plate of cast iron, sloping at the upper part.

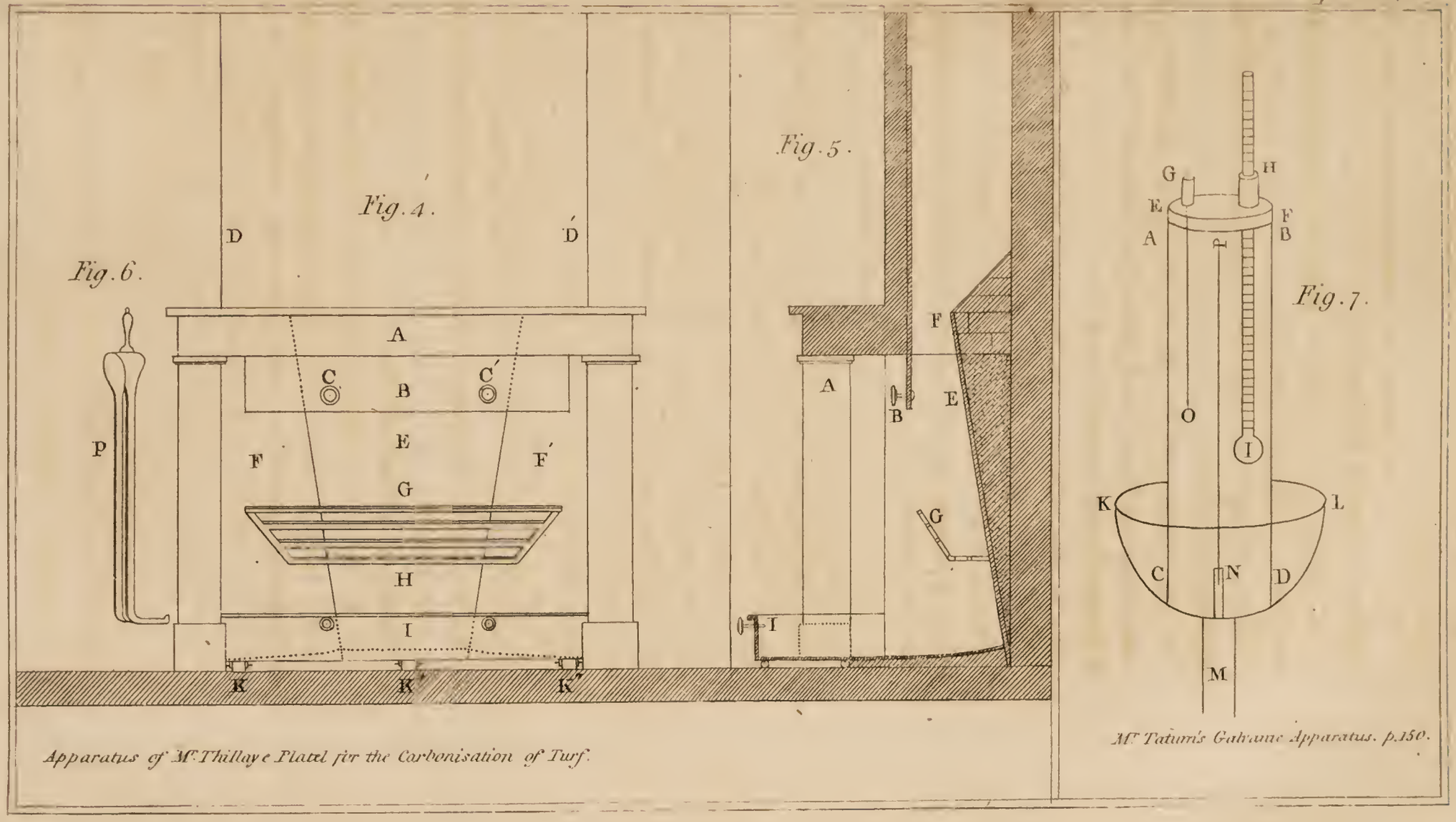
D, D′, the mantle-piece.

F, F, the sides, forming an angle of 135° with the bottom.

G, a kind of box, consisting of two grates; the interior one forming a pretty open angle with the bottom of the fire place; the other, making the fore part of the box, and consisting of two or three parallel bars. By this arrangement the turf, which requires only a very rapid current of air to burn it, is isolated.

H, the length of the bottom of the box, formed like the other part of common cast iron.

I, the



Apparatus of Mr Thillaye Platel for the Carbonisation of Turf.

Mr Tatum's Galvanic Apparatus. p.150.

I, the ash-pit, the bottom of which is a little arched toward the farther part, so as to give it greater depth.

K, K′, K″, the castors of the ash-pit.

Fig. 5. The profile or vertical section of the fire place.

A, the mantletree.

B, the anterior plate or regulator.

E, D, the posterior plate, of cast iron, curved so as to leave an opening for the smoke four inches broad by sixteen or at least fourteen long.

It must be observed, that this plate does not reach quite to the top, and that the line is continued by loose bricks, to facilitate the passage of the chimney sweeper; in addition the interval E is filled with a mixture of powdered charcoal and mortar or clay.

G, shews the shape of the grate into which the turf or charcoal is put.

Fig. 6. P, a pair of cranesbill tongs.

VI.

Method of curing Damp Walls, by the Application of a Composition newly invented by Mr. Charles Wilson, *of Worcester Street, near Union Hall, in the Borough.**

SIR,

I BEG leave to lay before the Society of Arts, &c. a cement, which, I trust, will be found of great utility in curing damp walls, in flooring damp kitchens, and for various other purposes, where the prevention of wet is necessary. Cement to keep out wet from walls or floors,

This cement when put in water will suffer neither an increase nor diminution in its weight: and it has the peculiar advantage of joining Portland stone, or marble, so as to make them as durable as they were prior to the fracture. and join stone or marble.

I have the honour to be,

Your very humble servant,

CHARLES WILSON.

* From the Transactions of the Society of Arts, who voted a premium of ten guineas to the inventor.

Receipt

Receipt for making the Cement.

The cement.

Boil two quarts of tar with two ounces of kitchen grease, for a quarter of an hour, in an iron pot. Add some of this tar to a mixture of slaked lime, and powdered glass, which have passed through a flour sieve, and been dried completely over the fire in an iron pot, in the proportion of two parts of lime, and one of glass, till the mixture becomes of the consistence of thin plaster.

The cement must be used immediately after being mixed, and therefore it is proper not to mix more of it at a time than will coat one square foot of wall, since it quickly becomes too hard for use, and continues to increase its hardness for three weeks. Great care must also be taken to prevent any moisture from mixing with the cement.

For a wall which is merely damp, it will be sufficient to lay on one coating of the cement, about one eighth of an inch thick; but should the wall be more than damp, or wet, it will be necessary to coat it a second time.

Plaster made of lime, hair, and plaster of Paris, may be afterwards laid on the cement.

Mrs. Ann Kemmish, King Street, Borough; Mr. Boone, Gregory Place; and Mr. Thomas Cannadine, Hook's Gardens, Tooley Street, have certified that Mr. Wilson's cement has been used with effect, on damp walls belonging to them.

VII.

Experiments on the Effects produced by a High Temperature on the Animal Economy. By F. F. Delaroche, *of Geneva.**

Living beings have a peculiar power of resisting cold,

AMONG the numerous characters that distinguish organized bodies, and particularly those of animals, from inanimate substances, one of the most remarkable beyond question is the faculty they have of resisting cold, and preserving in general a temperature superior to that of the medium in which they are placed. Accordingly this property of

* Journal de Physique, Vol. LXIII. p. 207. Sept. 1806.

living

living bodies has attracted the attention of physiologists in all ages, who have invented a thousand hypotheses, more or less probable, to account for it.

and also heat.

It has not been the same with the faculty enjoyed by animals, and perhaps by plants likewise, of resisting heat, and preserving a temperature inferior to that of the circumambient medium. Scarcely any researches on this head were made previous to the eighteenth century, when the invention of thermometers had enabled the philosopher to measure the heat of bodies with accuracy. The first experiments that were attempted might have led us to doubt the existence of such a faculty. Fahrenheit and Provoost, at the suggestion of Boerhaave, exposed three animals in a sugar baker's oven, the temperature of which was 146° F. One of these animals was a dog weighing 10lbs. one a cat, and the third a sparrow. All these died, the cat at the expiration of seven minutes, the other two in twenty-eight. These experiments were undertaken to verify a theory of Boerhaave's respecting the use of respiration. He had supposed, that it served, by the access of fresh air, to cool the lungs; in which, according to him, the blood underwent a fermentation, that produced a very considerable degree of heat. From the result of this experiment he thought himself authorized to conclude, that his theory was well founded, and that no animal could live exposed to a heat higher than its own temperature.

First experiments.

Dog, cat, and sparrow, killed by a heat of 146°,

in 7 or 28 minutes.

Boerhaave's theory of respiration.

The opinion of Boerhaave seems to have been generally adopted for a certain time by physiologists. It does not appear, that any precise notions of the temperature of hot climates were entertained at that time; but afterward more accurate ideas of it were formed, which did not agree with the law established by Boerhaave. In 1748, Dr. John Linings of Charlestown, giving an account of the meterological observations he had made in that place, noted the high temperature observed there in summer. Fahrenheit's thermometer in the shade frequently rose to 85° or 90°; and once he saw it as high as 98°. Though he did not examine the temperature of places exposed to the sun, he estimated with much probability, from other observations made in lower temperatures, that it must have been 124°. Adanson,

Temperature of hot climates.

At Charlestown 85°, 90°, and 98°.

In the sun 124°.

son, in his account of his voyage to Senegal, makes some observations on the heat he had experienced in that country. Among other facts he relates, that, in an excursion he made in a small vessel on the Niger, the temperature of the cabin in which he remained was from 115° to 125°, and did not fall below 99° during the night. In 1758, Mr. Henry Ellis, governor of Georgia, communicated to the Royal Society a fact respecing the excessive heat he experienced that year at Savannah. The thermometer in an open room facing the north rose to 102°. He likewise says, that going abroad with an umbrella, to screen him from the sun, a thermometer, which he held in his hand, rose to 105°: and that the same thermometer, when applied to his body, to his great surprise fell to 97°.

In Senegal, 115° or 125°, and at night 99°.

In Georgia 102°, and 105°, yet that of the body only 97°.

Observations respecting the temperature of vapour baths contributed likewise to shew, that man can support the action of a temperature superior to that of his own body. Such are those of Gmelin *, who observed, that the heat of the Russian vapour baths rose to 108°, and even 116° F.

Russian vapour baths, from 108° to 116°.

Some experiments on animals by Arnold Duntze † afforded similar results with respect to them. Dogs confined in a stove were capable of supporting a temperature of 106°, or even 108°, for a considerable time. It is true, however, they died, when the heat was raised to 113° or upward.

Dogs supported 106° or 108°, but killed at 113°.

Haller, in the second volume of his Elements of Physiology, has collected these and other similar facts, from which he concludes, that both men and animals, under certain circumstances, can support a temperature superior to that of their own body; adding, that in one or two cases the persons, who had observed this fact in themselves, had likewise remarked, that their own temperature kept itself below that of the surrounding medium.

Animals then support high temperatures, and their bodies are colder than the air around them.

In 1760 Tillet and Duhamel had an opportunity of seeing at Rochefoucaut in Angoumois a baker's maid-servant go into an oven, the temperature of which was at least 264°, and stay there about twelve minutes, without much incon-

A girl could stay 12 minutes in an oven at 264°.

* Flora Sibirica, t. I. pref. p. 81.

† Arnoldus Duntze, Experimenta Calorem Animalium spectantia, Leyden, 1754. Quoted by Haller.

venience.

venience. After they were gone, a person who had been present at this experiment repeated it several times at their request with another girl, employed in attending the same oven, and the results were the same. It is to be observed, that a spirit thermometer was used on this occasion, which gave the temperature of the oven only by approximation. Tillet considered the result of these experiments as militating against that of Boerhaave's. It appeared to him astonishing, that animals should have been destroyed in so short a time by a temperature of 146°, while women could support a temperature of 264°: and he inferred, that the speediness of the death of those animals must be ascribed to some cause foreign to the heat, such as the vitiation of the air in which they were included. In consequence he made some experiments, to ascertain how far Boerhaave's opinion was well founded; who, in consequence of his theory of the use of respiration, attributed the fatal effects of the heat to its action on the lungs alone. He exposed some animals in an oven heated to 156° or 166°. First he put them in naked, and let them remain some time: then, having taken them out, and allowed them time to recover themselves, he wrapped them up in linen cloths, which covered the whole of their body, and put them in again. In the latter state they supported the heat much better than in the former. Hence he concluded, that the heat does not act on the organs of repiration alone, but has a general effect on the whole body.

Another did the same repeatedly.

Tillet's remark.

Exposed animals to 156° or 166°, which they bore, but much better when clothed.

Franklin, in a letter which he wrote to Dr. Linings, published in the Journal de Physique for 1773, after giving an account of the researches he made in respect to the refrigeration produced by the evaporation of fluids, endeavoured to explain by this property a fact, which he had formerly observed in himself. On a summer's day, the temperature of the air being 100°, he had remarked that his own temperature was only 96°. He was at the time lightly clothed, and in a profuse perspiration. The reason of this difference of temperature he imagined therefore, to be the evaporation going on from the surface of his body.

Franklin observed the heat of his body less than that of the air,

and ascribed it to evaporation.

In 1775, Dr. Fordyce joined with Sir Joseph Banks, Sir Charles Blagden, Dr. Solander, and some other natural philosophers,

Experiments by Dr. Fordyce, &c.

losophers, to make fresh researches into the influence, that high temperatures have on the animal economy. Their experiments are too well known, to be repeated here: it is sufficient to say, that they could support for several minutes, without being too seriously inconvenienced, a heat superior to that of boiling water; and that they confirmed, in a more accurate manner than had before been done, the faculty man enjoys of keeping himself at a nearly constant temperature, though placed in an atmosphere of which the heat is far superior to his own. These gentlemen, struck with the copious perspiration, that was formed when they were exposed to the heat; observing too, that the moment when this perspiration shewed itself was distinguished by a diminution of the painful sensation they experienced from the heat; were led to suppose, that the evaporation from the surface of the body contributed greatly to this uniformity of temperature. Some experiments they made on the heating of liquids exposed in open vessels, and introduced into the heated room, confirmed them in this opinion. In fact these liquids kept themselves uniformly at a temperature below that of the surrounding medium, and could not be brought to boil, till they were covered by a stratum of melted wax, which prevented the evaporation. Nevertheless these gentlemen did not think, that evaporation of the perspirable matter was the sole cause of the uniformity of temperature, which they had observed in themselves, though exposed to a heat so much higher.

Supported a heat above 212° some time.

Ascribed it to evaporation,

which appeared to keep liquids under similar circumstances from boiling.

They supposed however some other cause.

Dr. Dobson made similar experiments.

At the same time Dr. Dobson, of Liverpool, made some experiments in the hospital there, which were nearly similar and attended with similar results.

J. Hunter applied heat to parts of the body,

and imagined its effects were resisted better than those of cold.

About the same period too, or a little after, Mr. John Hunter published some inquiries he had made respecting the heat of animals. Most of these related to their faculty of enduring cold; some however respected their capacity for resisting heat. The latter were not made on the whole body of men or animals subjected to the experiment, but on particular parts merely; and Mr. Hunter thought he perceived, that this faculty, though it could not be considered as absolute, was more decidedly marked than that of resisting cold.

When Sir C. Blagden's first paper appeared, Mr. Changeux made some remarks on it.* He particularly endeavoured to prove, that it was not by virtue of a particular property, that the human body resisted the effects of heat, but from causes purely physical. These causes, according to him, were on the one hand the evaporation of the perspirable matter; on the other the refrigeration of the air introduced into the lungs, the effects of its rapid passage through the trachea. He does not appear however, to have made any experiment on the subject.

Changeux ascribed it partly to evaporation, partly to the cooling of the air in its passage to the lungs.

In 1779, Dr. Crawford, in the first edition of his work on animal heat, promulgated the opinion, that the faculty possessed by animals of producing cold depended solely on the evaporation of the perspirable matter pulmonary and cutaneous. Subsequently, in a paper in the Philosophical Transactions, and in the second edition of his work, he advanced a contrary opinion, founded on some experiments of which I shall give an account in another place.

Dr. Crawford, at first to evaporation solely from the skin and lungs, afterward in part to less oxigen being absorbed from the air.

Having observed, that animals exposed to heat vitiated the air less by respiration than such as were exposed to cold, he thought he could explain by this fact the faculty of producing cold which they possess. I shall not attempt here to give an account of the theory he invented on this point, a theory which I confess I do not very well understand.

Such are the principal researches and observations, that, to the best of my knowledge, have been published respecting the influence of heat on animals. The subject however was far from exhausted, as several questions remained undecided, and others even wholly neglected. A few inquiries, however incomplete, that I have made myself respecting it, will form the conclusion of this essay. They are far from filling up the chasms that were left; but I shall deem myself happy, if they throw some light on a few points, and meet the indulgence of the enlightened judges, to whom I submit them.

It is incumbent on me to add, that the experiments, which constitute the base of these researches, are not exclusively my own; they were made in concert with my friend, Dr.

* Journal de Physique, t. VII. p. 57.

Berger,

Berger, of Geneva, who shared the labour, and assisted me with his advice.

SECT. I. *Of the Degree of Heat that Animals can endure.*

The limits of this faculty not determinable with precision.

It is scarcely possible to investigate the effects produced on animals by heat, and the faculty they have of resisting it, without being prompted to ask, what are the limits of this faculty? in other words, what is the greatest degree of heat they can support, without being deprived of life?—This question however is insusceptible of a precise answer.

The time must be considered as well as the intensity.

The effects of heat being in the ratio of the duration as well as intensity of its action, it is not till a very long time has expired, that we can consider an animal as having undergone all the influence of the heat to which it has been exposed, and conclude it to be capable of resisting it.

The temperature subject to variation,

We cannot likewise prevent this temperature, on the one hand, from experiencing considerable variations, which hinder its being ascertained with precision; and the animal, on the other, from being subjected to the influence of foreign circumstances, by which the effects of the heat will be modified.

and the animal affected by various circumstances.

Conclusions from the author's experiments.

The author here relates various experiments made on several animals, from which he draws the following conclusions.

From these experiments it follows, as might have been presumed, that all animals are not equally affected by heat, and that the faculty of resisting it is not the same in every species. We cannot therefore derive from them any general and precise conclusion with respect to the measure of this faculty.

Small animals killed by a heat of 134° or 144°.

These experiments however are sufficient to shew, that most animals, at least those of a small size, sink under a temperature of 144°, or even 134°, after a certain space of time, which is generally pretty short. They shew too, that the progress of the symptoms is more rapid, and the arrival of death more speedy, in proportion as the heat is greater.

The larger the animal the better it resists heat.

The size of animals appears to have a marked influence on the speediness of the effects of heat. The ass supported them much longer than the cat, the dog, the rabbit, and the guinea-pig; and these longer than the mouse. The magpie

magpie and the bunting were killed sooner than the cock or the pigeon. The difference was scarcely less striking between a large and a small frog, a beetle (*scarabæus nasicornis*), and a wood-louse. It was not the same however in all cases. The guinea-pig, though less, appeared to support heat a little better than a rabbit exposed to the same temperature; and the sparrow lived longer than the cock and the pigeon. Exceptions.

The results were not less modified by difference of organization. Frogs and cold blooded animals supported heat much better in proportion to their size than hot blooded animals. The larvæ of beetles, leeches, and fresh-water snails (*bullæ fontinales*), though still smaller, supported it equally well. It was not the same with beetles in their perfect state, mole-crickets, and wood-lice, which were killed much more quickly. Cold blooded animals endure heat best. Other exceptions.

(To be continued.)

VIII.

On the Increase of Temperature produced by the Galvanic Action. By Mr. John Tatum, *Jun.*

To Mr. NICHOLSON.

Dear Sir,

In the paper I sent you about two or three months ago, on the rise of the temperature of water during its decomposition by galvanism, I proposed sending you the results of other galvanic experiments I had made near a twelvemonth since: but having mislaid the minutes I took during the experiments, and various avocations preventing my repeating these until the present time, is the cause of my not fulfilling my intention so soon as I wished. Additional experiments on galvanism.

In the following experiment I had two objects in view, the one was, to ascertain the temperature to which the water rose during its decomposition: the second, to confirm the faint recollection I had of muriatic acid being formed in the experiment alluded to: for which purpose I made use of two troughs, each 26 plates, each plate 50 inches surface; and

two

two troughs, each 25 plates, each plate 36 inches surface; with diluted nitrous acid I had made use of four days before, now adding a little more acid.

Description of the apparatus.

As the apparatus I made use of to contain the water is one of my forming and making, it will perhaps be necessary to describe it, before I relate the result of the experiment.

Let A B C D, *Pl.* IV. *Fig.* 7. represent a glass tube capable of containing $1\frac{1}{2}$oz. of distilled water; E F a brass cap, through which passes a screw G, to which can be attached a platina or other wire; and by taking the screw out another sort of wire may be fixed, as is represented by O: H a box with leather through which the thermometer I passes, and then screws on tight, graduated on the tube; K L a basin, in which the tube rests when filled with distilled water; M a neck, which may be fitted in any convenient place to support the apparatus, screwing through the basin, and terminating in a pair of forceps, into which may be inserted any sort of wire.

On forming a connection at G with one end of the battery, and at M with the other end, the galvanic fluid will pass through, and decompose the water in the tube; and the thermometer will indicate the temperature.

Having explained the apparatus, I proceed to the experiment.

The zink end of my battery communicated with G, which was provided with a platina wire O. The inferior platina wire P was connected with the copper end of the battery.

Elevation of temperature 26 degrees.

A stream of gas was projected downwards half an inch from the lower end of the wire O. The wire P became oxided very fast; and the thermometer, which was at the commencement of the experiment 54°, rose to 80°. When I had decomposed $1\frac{1}{4}$oz. of water, I disengaged the apparatus, and tested the water (which was forced from the tube into the basin) with the nitrate of silver, which gave a white appearance.

Muriatic acid.

From this appearance I conclude muriatic acid was formed; but what furnishes its constituent parts? surely neither the glass tube, the platina wires, nor the basin. As we cannot suppose that either of these furnished it, we must look for its

ts component parts in the distilled water, which is well known to be composed of oxigen and hidrogen; and oxigen being considered as the acidifying principle, I conceive, that the muriate formed is an oxide of hidrogen, but in such proportions as constitute an acid.

In hazarding this opinion I know I differ from, I believe I may say, all of the most reputable modern chemists, such as Thomson, Fourcroy, Accum, &c. who, when speaking of acids, say 'the base of muriatic acid unknown'; and also, that 'oxigen forms no other combination with hidrogen, than that which constitutes water'. I hold these authors in the highest respect, and derive the greatest information from their works, which I conceive do them great credit; but facts are stubborn things, and I make it a rule, to bow my theory to truth.

I am, Dear Sir,

Your most obedient,

JOHN TATUM, *Jun.*

April 14, 1807,
Dorset Street, Fleet Street.

P.S. At first I expected the mercury in the thermometer to rise much higher, but, owing to using the diluted acid a second time, the water was decomposed much slower than in the experiment in my former paper; this, added to the apparatus being considerably larger, much more metal about the cap, and the thermometer having a largish bulb, all of which either absorb or conduct off the caloric, will account for my disappointment.

SCIENTIFIC NEWS.

On the Tempest of Feb. 18, *which has produced many dreadful accidents in the Channel.*

(Continued from p. 88.)

Observations by la Marck on his system concerning the influence of the moon upon the weather.

No one can read the detail of the numerous shipwrecks which are mentioned in letters from Havre, Dunkirk, Dieppe, St. Valery, and Calais, without being deeply afflicted. Further details from Laon, Bruges, Ghent, and Paris, augment the melancholy

melancholy list of incidents. Surely, it is high time, that the causes which produce such dreadful events were taken into serious consideration, and that an inquiry into them should receive the attention and interest to which it is entitled.

Paris, Feb. 25, 1807. LAMARCK.

The following letter from M. de Lalande to the editor of the *Moniteur* has been inserted in that paper of the 1st March 1807:

Remarks on the same by La Lande.

It does not appear in any wise probable to me, that the passing of the moon through its *nodes* produces any sensible change in the atmosphere, as M. De Lamarck thinks: but its passing over the equator is more observable; I have noticed it many times; and even this year, in the months of January and February, there have been alternations of cold and heat, which appeared to follow the passings of the moon over the equator. For that reason, I have marked them in the annuary of the Board of Longitude, from the beginning.

But the dreadful hurricane of the 18th February can have no relation to the moon. These phenomena proceed from the winds, from thunder, and from volcanoes, or swellings of the sea. We may hereafter learn, perhaps, that on the 18th February, there have been violent thunder storms in some of the southern provinces, and I should wish to be informed of it through the *Moniteur*, a paper in which scientific men like to deposit their observations and remarks.

(Signed) DE LALANDE.

Fig. 1.

Theory of Looming or Horizontal Refraction.

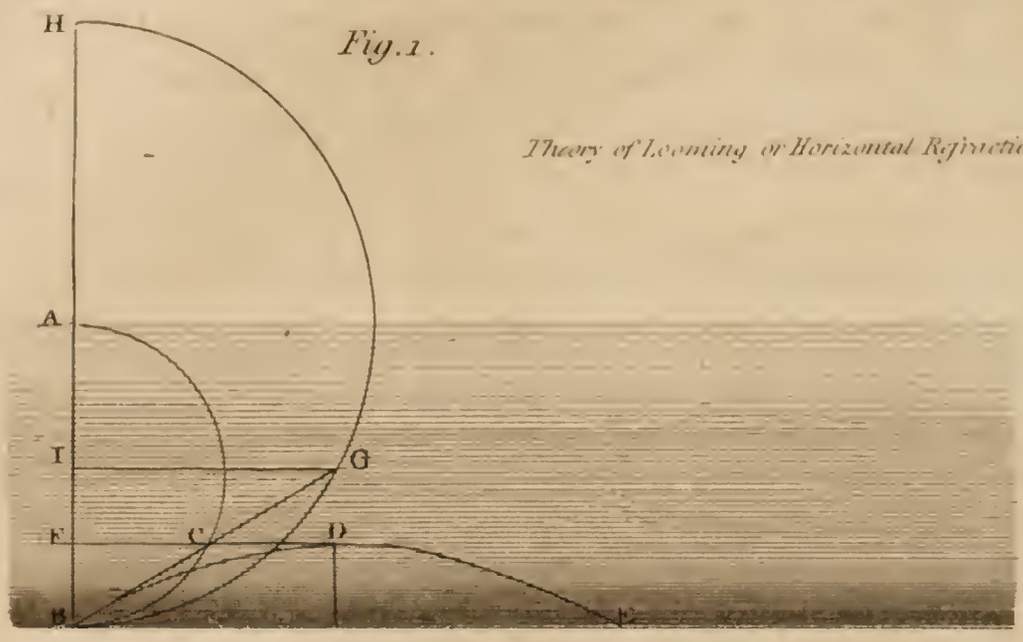


Fig. 2.

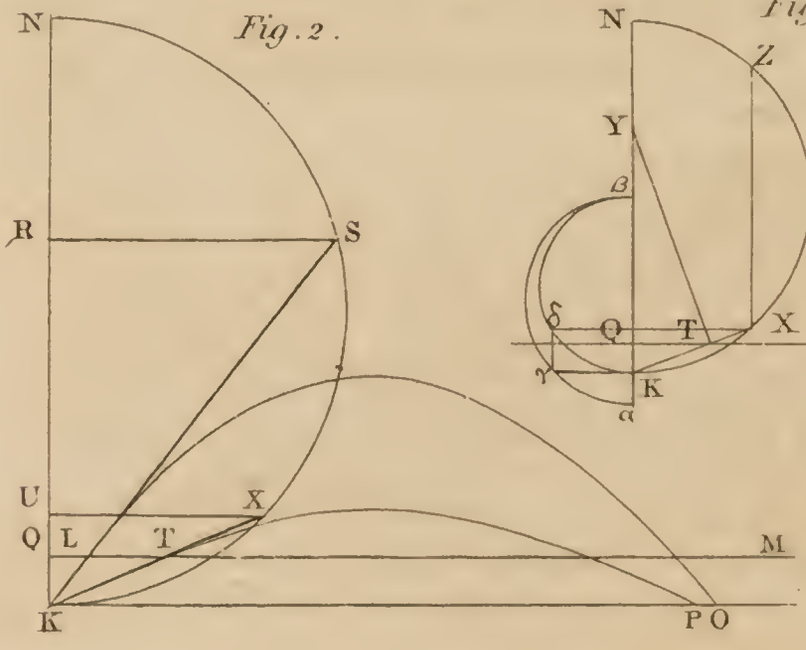


Fig. 3.

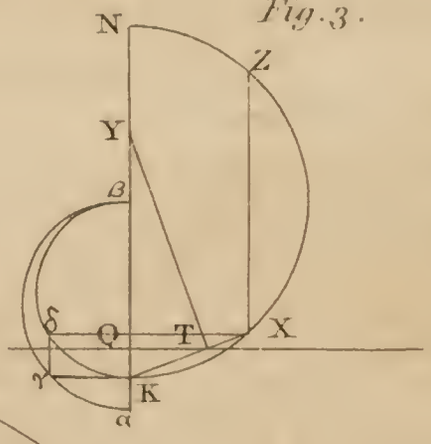
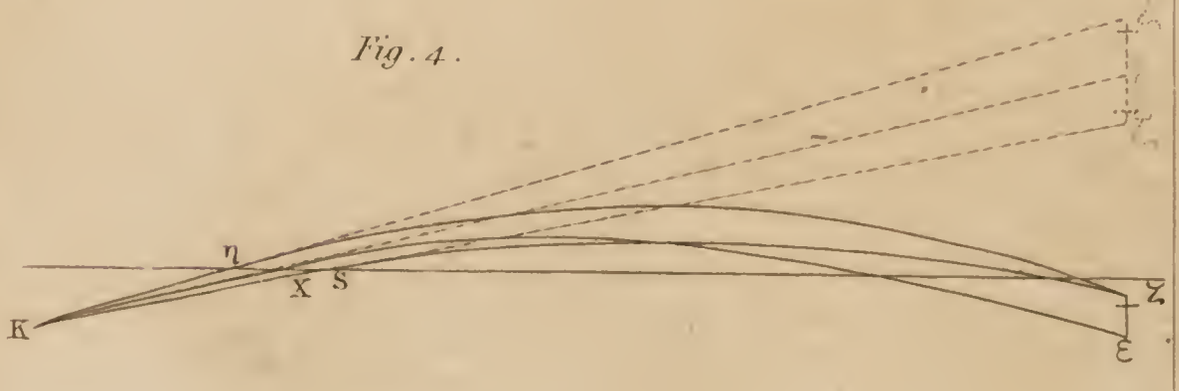


Fig. 4.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1807.

ARTICLE I.

To Mr. NICHOLSON,

SIR,

NOTWITHSTANDING the very ingenious inveſtigations of Dr. Wollaston and others, it appears to me, that the subject of looming, or horizontal refraction, is still capable of being explained with greater precision, and upon simpler principles: I shall therefore trouble you with a few observations, which have occurred to me respecting it.

Looming, or horizontal refraction.

Let the refractive density of a medium be supposed to vary gradually and equally, in parallel strata; the variation beginning from a certain plane surface, and being continued, till, at a certain distance above that surface, the refractive power wholly vanishes. For example, the refractive density of air being expressed by 1.0003, if the temperature vary 1° in 1 foot, the refractive power will vary .000 000 6; and dividing 1.0003 by this, we have 1 666 667 feet, for the imaginary height of a medium continuing to vary at the same rate till its refractive power vanishes.

Suppose strata regularly diminishing in refractive power.

Now upon the projectile hypothesis, supposing a particle of light to be initially at rest in this medium, it will be actuated by a constant accelerating force; and by falling from the top to the bottom, it will acquire the velocity natural to light in the original medium: and if a ray of light enter

Progress of light through it.

the variable strata from the medium, its motion will be similar to that of a jet or a projectile rising in any direction from the bottom of a reservoir with the velocity due to its height.

Path of the ray of light.

Let A B (Pl. V. Fig. 1.) be the imaginary height; if we describe the semicircle A C B, a ray of light entering at B, in the direction B C, will describe the parabolic path B D E, B E being four times F C; or if the circle B G H be twice as great in diameter as A B, F D will be equal to G I. And if several rays, passing from a point K, (Fig. 2.) enter the variable medium at the lower surface L M; making the semicircle K N equal to B G H, the distances K O, K P, will be equal to 2 Q L + 2 R S, and 2 Q T + 2 U X, respectively.

Now the distance K P must be a minimum, when the fluxions of Q T and U X are equal; that is, when K Y = X Z, (Fig. 3.) T Y being perpendicular to K T, and X Z parallel to K N. Make K α = K Q, and describe the semicircles K β, $\alpha\beta$, K β being half of K N; draw K γ perpendicular, and $\gamma\delta$ parallel to K N; then δ X, parallel to Q T, will determine the position of the point X so as to fulfil this condition. It is obvious that when K Q is very small in proportion to K N, δ X will coincide with Q T, and X will be in the intersection of the circle K N with the surface.

Apparent place of the object.

Consequently to an eye placed at K, (Fig. 4.) the object ε will be seen in the direction K X, and the object ζ in the directions K η and K ϑ; so that there will be two elevated images of the line $\varepsilon\zeta$, the one erect, and the other inverted.

Double.

An inverted object only.

If the variable medium be only thick enough to admit the passage of the rays below K X, there will be no direct image, but an inverted one only. The inverted image will in general be nearly of the natural dimensions, although a little contracted; the case being nearly similar to a very oblique internal reflection. The points K and ε may be considered as conjugate foci, with respect to the refraction of the variable medium.

In the supposed case of the variation of a degree for each foot of air, K N being $3\frac{1}{3}$ million feet, if K Q be 1 inch, K X will be 527 feet, and K ε 700 yards. The angular deviation

deviation of the place of the point ε would be 32 seconds: the passage of the light through every 16 feet of the medium producing a total deviation of a second; and if the change of the air's density were more or less than $\frac{1}{500}$ in the space of a foot, the deviation would be as much more or less than a second in each space of 16 feet through which the light passes. The curvature of the earth's surface becomes a second in 102 feet; consequently a change of density amounting to $\frac{1}{3200}$ in a foot, or a change of temperature of a degree in 6 or 7 feet, would be sufficient to produce a refraction equivalent to the apparent depression of a distant object arising from this cause, and to elevate the coasts of a wide channel, so as to make them visible to each other. This result may also be more simply obtained from Simpson's investigations respecting atmospheric refraction, the refractive density being inversely proportional to the distance from the centre of the earth, when the temperature varies 1° in 6 or 7 feet; for, as Dr. Young observes in his extensive system of natural philosophy lately published, Vol. II. Art. 461, "If the refractive density of a medium vary as a given power of the distance from a certain central point, the angular deviation of a ray of light will be, to the angle described round the centre, as the exponent of the power to unity." Proportion of the deviation.

I am, SIR,

Your very obedient servant,

EMERITUS.

Postscript. If it be required to determine the position of X for the lowest ray that can cross the line ζ ε, supposing it to be at any other distance from K, we must make the rectangle N U X = ¼ N K ε; as may be understood by considering that the fluxion of the tangent of P K X is inversely as N U, and the fluxion of K U is as U X. This determination requires in general the solution of a biquadratic equation; but when K ε is very small in proportion to K N, U X will be very nearly ¼ K ε, or still more nearly ¼ K ε + K ε cub. ÷ 64 K N q. The point ε thus found will be the single point of the image as before: the length of the path of the ray within the variable medium will in both cases be Formula for determining the place of the image.

Formula.

half of the distance K ε; but the total deviation of the light will not be twice the angular displacement of the point ε, unless K and ε be equidistant from the surface. If however the angular direction of the surface be known, as is almost always the case in nature, the angle P K X, which is half the deviation, may also be found by observation; being, for example, when the surface is horizontal, the actual angular elevation of the image of the point ε. The place of the surface L M, which limits the variable medium, may be found from the measures of the actual elevation and the displacement of the point ε; for its distance from *t*, the middle point between ε and its image, is always one fourth of the elevation. The circumstances will be nearly similar when K is either in the line L M, or a little above it, provided that ε be below it; but if both these points are above it, there will be no double image.

Variation of the medium continued to the eye and the object sometimes.

If however, the variation of the medium be continued, in an inferior degree only, to the place of the eye and the object, effects of a similar nature may still be sometimes produced: but it is not sufficient in this case to suppose with Dr. Wollaston, that the curve indicating the density has a contrary curvature; for it must be such, that the change of density, and consequently the curvature of the ray, must vary more rapidly than the distance from the line joining the eye and the object; for example, if the curve be logarithmic, its subtangent must be considerably less than the greatest distance of the bent ray from its chord; otherwise there can be no double image. Supposing the curvature of the rays be as the distance from any given line, the form will be nearly that of the harmonic curve. But whenever the object can be seen in its true place, beside the appearance of one or more displaced images, it is obvious that both the eye and the object must be situated in a uniform medium, as we have hitherto supposed.

Object seen in its true place.

Fig. 1.

Fig. 2.

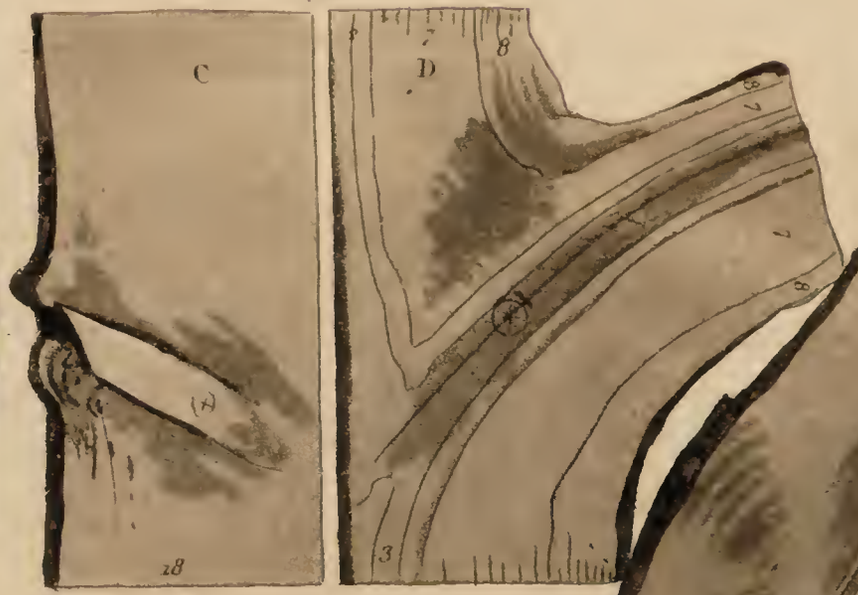


Fig. 3.

Fig. 5.

Fig. 4.

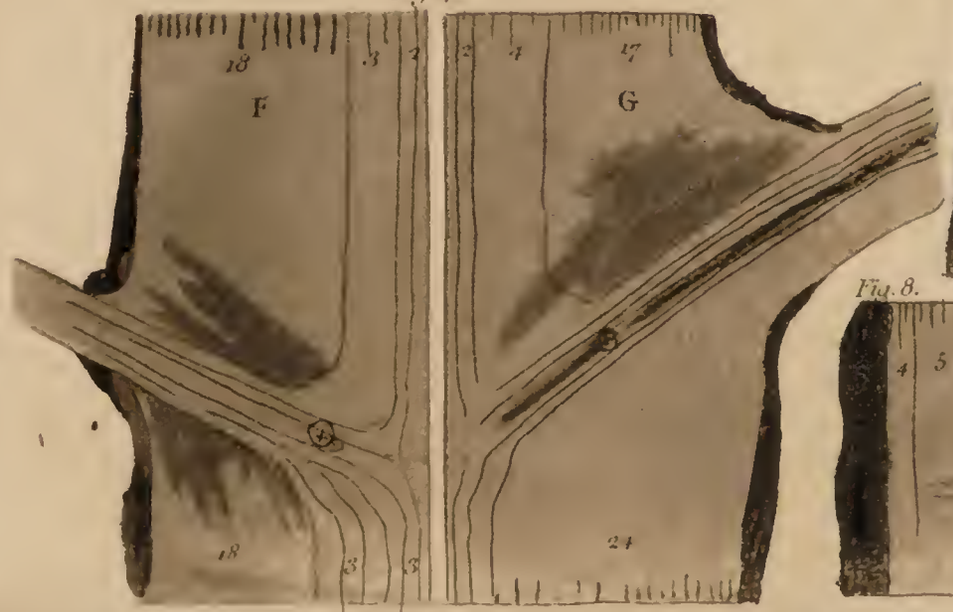


Fig. 6.

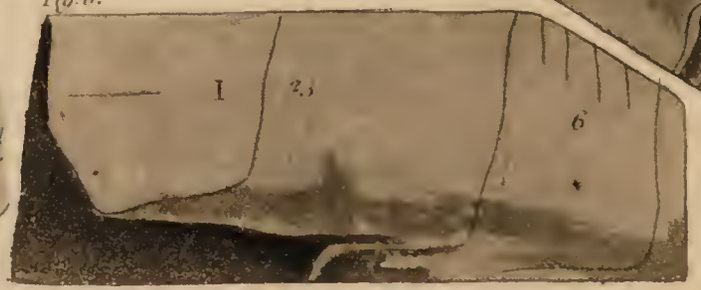
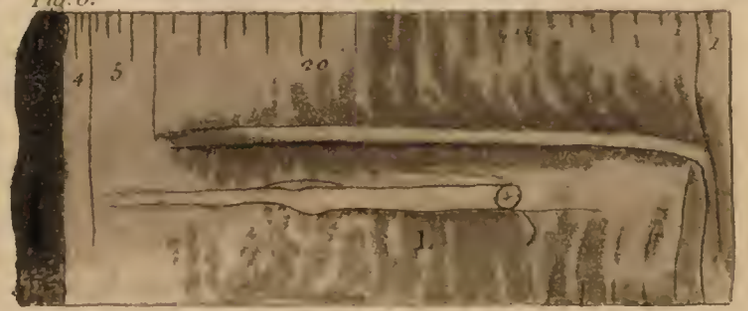


Fig. 7.

Fig. 8.



II.

Remarks on Pruning Fir Trees; with an Engraving, to explain the Advantages of the Method recommended. By Mr.* ROBERT SALMON, *of Woburn, Bedfordshire.*

SIR,

I HAVE the honour of transmitting to you some observations on the management of fir plantations. Having had the care for some time past of such plantations, and knowing how much they are increased in this kingdom, I considered it as a matter of importance, that a proper mode of management should be generally known, in order to bring timber to the greatest attainable perfection. For this reason I have turned my thoughts to the subject, and am confident that much may be done, as is elsewhere asserted, by good management. I have collected several specimens to demonstrate the difference between good and bad management, and have made some observations, which I have not before met with, and may perhaps be useful; you will have the goodness, therefore, to present these observations and specimens to the Society of Arts, and to believe me,

Plantations of Firs much increased.

Your very humble servant,

Woburn, April 29, 1805. ROBERT SALMON.

To Dr. C. TAYLOR.

References to Plate VI. showing specimens of English grown Fir Timber, cut out of his Grace the Duke of Bedford's plantations at Woburn, pointing out the impropriety of leaving timber to the course of nature, and the loss and defects that arise from such mode of management; also illustrating the necessity of some fundamental rule for managing the same, and the advantage of early and close pruning off superfluous branches, with a general rule for

Necessity of early and close pruning.

* Transactions of the Society of Arts for 1806.

performing

performing the same, and regulating the distance of the plants in Fir Plantations.

Fig. 1. Section A. shows a dead knot and progress in the growth of the tree, having 19 years of growth below the bough, and 18 years above it. From the regular course of nature, as shown by this section, it is evident, this bough or knot must have existed as long as the upper part of the tree, namely, 18 years. For the first three years the growth and accumulation of the bough proceeded regularly with the tree; but about that time (now 15 years ago) the bough must have been distantly cut off, thereby preventing its regular increase in the part left remaining; for 6 years after cutting off it appears to have barely existed, and after that ceased to exist at all but as a dead bough. Since it became so, 9 years of accumulation have taken place on the trunk of the tree, thereby gradually enclosing a part of the dead bough, which part so enclosed is what by joiners is properly called a dead knot; the boughs that exist and are enclosed whilst living, are the live knots, and these the tree will produce either as the bough may be distant or close cut from the tree. From this specimen may be determined, that if the bough had been cut close to the tree at four years growth, there would now have been sound clean wood over it to the outside: or when it was cut, if it had been taken off at ⊕, sound clean wood would have formed over it to the outside.

Bough left too long.

N. B. In all the specimens this mark ⊕ is affixed to point out the proper place for cutting off, and is so placed as to allow for thickness of bark at the time it should have been cut.

Section B. shows a striking instance of the impropriety of leaving the smallest bough cut at a distance from the tree; this bough was cut off and became stagnant at 2 years growth, notwithstanding which it was 14 years before the wood on the trunk accumulated to the end of the dead knot.

Other instances of bad pruning.

Had this bough been cut at ⊕, the knot as far as that would have been firmly united with the tree, and above it all sound clean wood.

Fig. 2.

Fig. 2. Section C. Another instance of improper cutting. Bad pruning.

If it had been cut at ⊕, the timber would have been more valuable.

Section D. This before cutting exhibited a healthy bough, and the section shows it the same, exhibiting a live knot. This specimen clearly shows the progress of nature in healthy boughs; it also shows the great impropriety of suffering such boughs to exist more than 5 or 6 years, at which age had it been cut off, instead of a knot and great defect there would have been clean wood from ⊕ to the outside.

Fig. 3. Section E. Another striking proof of the impropriety of long cut boughs, a dead knot of many years standing, but far from being enclosed now, admitting wet into the heart of the tree: it should have been cut at ⊕.

Fig. 4. Part of another Scotch fir.

Section F. A very striking proof of young and bad pruning. This bough was cut at 4 years growth, and now, after 18 years accumulation of the trunk, remaining uncovered, and would so have remained many years longer.

Section G. A bough which, whilst standing, appeared not vigorous or healthy; the section shows that at 6 years it was in decline, and after that increased very little, though its increase may be distinctly traced to the present time.

This should years ago have been cut off.

Fig. 5. A very complete specimen of good pruning, though much too late. Instance of good pruning.

Section H. A large bough cut off at 6 years growth, but so close cut, that in 4 years afterwards the wood on the trunk of the tree is arrived at the extremity of the knot.

Fig. 6. and 7. Parts of a Weymouth Pine Fir, 31 years growth, a most striking specimen, and complete refutation to the doctrine of those who contend that the best way is to leave plantations to prune themselves. This tree grew near the outside of a thick plantation securely fenced, and in a state of nature at the time it was felled, except some accidental breaking off a few boughs near the bottom of the tree. Before it was felled it indicated sickness by the foliage, but from what cause it was so, no trace appeared, as the trunk bore a very healthy appearance. The section shows, by the small Wood left to itself.

small increase of wood for the last 6 years, that it was not then healthy.

Section I. A dead knot from a bough broke off at 6 years growth; since which 25 years growth of wood have formed on the trunk, without nearly covering the stump; this stump was broken off before representation.

Fig. 7. is a horizontal section of three other knots cut from the same round as figure 6, showing the great obstruction to the growth of the tree round the knots: this also explains the cause of the great hollow round the knot in fig. 6.

The knot at K is a most striking and undeniable proof of the impropriety of leaving the smallest bough for nature's disposal. This bough protruded beyond the tree some distance, and evidently never existed but in a stagnant state, for the last twenty-nine years; it being only two years old when it so became stagnant.

Fig. 8. A piece of the same tree as fig. 6 and 7; but from the next higher tier of boughs, having a small piece wasted between the two.

Section L. Two small boughs, the upper one only one year's growth, was twenty years before covered, and has nine years wood over it. The lower bough is of six years growth, was twenty years before covered, and has four years wood over it.

Perfection of Fir.

On contemplating these specimens; considering the purposes that fir timber is generally applied to, and having some knowledge of plantations of this sort, it must occur, that clearness of knots, straightness, length and equal size of its trunk, constitute its perfection; and, if deficient in all these, it is of no value but for the fire. Next to these considerations, and the prospect of an improved knowledge of cultivating this article, it may be a fair question, if our own country is not capable of producing fir timber little or not at all inferior to the foreign fir.

Fir may be produced here equal, if not superior to foreign.

At present firs in this country appear not for any period to have been considered much otherways than as ornamental. For this purpose they serve but for a certain time, which past, it has been their fate to be cut down long before having attained maturity. But from the vast plantations now established, it is to be hoped, that another century may obtain

to

to English Fir some of the character of the English Oak; towards such end, if attainable, every means should be used, and towards it nothing appears more likely to succeed, than a well grounded general practical mode of management, from the time of their being planted out, to their greatest imaginable age of improvement. That a knowledge of such may by perseverance be gained, is not much to be doubted; and by inspecting and considering the specimens herein referred to, there appears great reason to conclude, that early and proper pruning and thinning will form a considerable feature in the system to be adopted.

Now as forms are first instruments in good systems, and as proceedings on fundamental principles (though in the essay they may a little err) are better, in a general view, than occasional success by hazard; so it may be warrantable that a system for general management may be laid down, although the author cannot possibly have lived to prove all by experience: so the rules hereafter submitted are given, being the result of only a few years observations.

Planting thick.

For planting, from every authority or observation, there can be no doubt that all firs should be planted thick; not more than four or five feet apart.

Not several species together.

Where firs of the same kind are planted together, there is less loss of plants from one sort overgrowing and destroying the others; consequently it appears adviseable, that all the different sorts be planted by themselves. If any admixture be at all admitted, the Scotch and larch may best succeed: but this is not certain, and they will certainly be best separate on two accounts; first, because they are not so likely to injure each other; and secondly, the larch may be put into the ground best suited to them, and the Scotch the same.

Spruce as a skreen.

In making plantations of any particular sort, it may be right to have a few spruce, or other sorts on the outside, to prevent mischief from sudden gusts of wind; but if the situation is not subject to such gusts, the spruce had better be omitted, being mechanical agents only, and by excluding the sun and air they act against the operation of nature.

Ornamental plantations.

In these hints ornament is not considered; if such be wanted, and profit also, then the spruce, larch, silver, and some others may be combined.

From

Rules for pruning.

From some years observations on pruning and the effects thereof, it appears certain, that Fir trees, at a certain age, should be pruned to a certain height; and for regulating thereof, the following simple rule is recommended. The pruning to commence when the trees are six years old, or when there is discernible five tiers of boughs and the shoot; the three lower tiers of boughs are then to be taken off. After this first pruning, the trees to be let alone for four or five years, and then, and at every succeeding four or five years, the pruning to be repeated, till the stem of the tree is clear to forty feet high, after which, as to pruning, it may be left to nature. The rule for the height of pruning, after the first time, to be half the extreme height of the tree, till they attain twenty years growth; and after that time, half the height of the tree, and as many feet more as it is inches in diameter at four feet from the ground. This pruning is known from repeated observations not to be excessive; and the rule is calculated to check the too tapering top, and strengthen the slender bottom, by carrying the pruning to a greater proportionate degree, in a ratio compounded of the height and bottom bulk; and by this rule it may be observed, that the trees will be at top clothed with somewhat less than half their branches. The proper time for pruning is between September and April, and the tool to be used, the saw.

Thinning.

Orderly thinning the trees at certain periods is the next essential to pruning, and for this purpose observations have been made on the most orderly and thriving plantations, and the following simple rule is recommended. Keep the distance of the trees from each other equal to one fifth of their height. In the application of this rule for thinning, it is evident, that each individual tree can never be made to comply; for the original distance (even if planted in the most regular order) will allow only of certain modifications, by taking out every other tree, and so on; but even if the obtaining such equal distance was practicable, experience would show that another way should be preferred, of which the eye must be the judge, by taking out such trees as are least thriving, stand nearest another good tree, &c.; at the same time keeping in view the rules prescribed: the following

ing of which rules may easily be proved by measuring a chain square, or any quantity of the land, and counting the trees thereon; then by trying the height of two or three trees in that quarter, and taking one fifth of such for the distance, it would be readily seen how many trees should be contained in the piece measured: or the practice may more simply be regulated, by taking the distance of eight or ten trees added together, the average of which should be equal to a fifth of the height of the trees.

In these rules nothing impracticable or complicated is proposed.

Produce of trimming more than pays expense.

The author has for years known the expense and produce from trimming only, and finds in Bedfordshire the produce doubly repays the expense; and although some experimentalists may differ from him, or time may show some reason for deviating somewhat from his rule, yet it is presumed all will agree that some simple system is adviseable, instead of having plantations and woods mismanaged, to the great loss of the community and the proprietors. If such a system as proposed be generally promulgated; if not perfect, it will most likely, in time, become so, and thereby have its advantage; and that some advantage may be had in speculation, the following concluding remarks are introduced.

Pruning garden trees.

In the common course of gardening, it is understood, that pruning invigorates the tree; that trimming off the side branches makes the upright ones shoot the stronger, and by cutting out the dead and decayed wood the tree is kept alive: some of this doctrine will certainly apply to the tribe of firs; it will certainly substitute clean wood for knots, and of all this treatment, from their particular uses, they of all other trees stand in most need, and will be most improved by it. And should it be admitted, that like treatment would on the fir, as well as other trees, produce the like effect, it would lead to a well-grounded expectation, that, as well as producing clearness from knots, straightness, and length, the same operation would advance the quality nearer to that of foreign fir; for it may be traced, that where trees are tall and clear of boughs or knots, the whole substance of the wood is better and of finer grain, and it appears likely, that such will always be the case: the reason may probably be inferred

Advantage of pruning Firs.

inferred from the sap having farther to rise and descend, and having no boughs to divert or delay it, the circulation must be more fine and rapid, most increase be left in the neighbourhood of the boughs at the top of the tree, and least on the sides at the lower part; consequently adding to the length of the head, and rendering more fine each annual increase to the body; thereby producing a close-grained, clean, long, and regular, easy-tapering, useful piece of timber; instead of a coarse-grained, short, sudden-tapering trunk, with a quantity of boughs and knots.

Applicable to other timber.

The foregoing observations and rules are meant to apply to fir timber only, but to a certain degree they may be applied to other timber; though by no means to the same extent, or age. But if applied as far as the first fourteen years of their growth, and then the pruning altogether omitted, and the thinning-out very much increased, any plantation would be rendered much more valuable, than if left entirely to nature.

ROBERT SALMON, Surveyor.

Woburn, April 2, 1806.

III.

Abstract of a Memoir read at the Meeting of the fifth Class of the Institute, September the 29th, 1806, *by Mr.* LAPLACE, *on the apparent Attraction and Repulsion of small Bodies floating on the Surface of Liquids* *.

Bodies floating on fluids attracted toward each other,

IN the theory I have given of capillary attraction, I have subjected to analysis the attraction of two vertical and parallel planes, very near each other, with their lower extremities immersed in a fluid. I have shown, that, if they be

when they are of the same matter.

of the same matter, this action tends to bring them nearer together; whether the planes elevate the fluid near them, as ivory immersed in water; or depress it, as laminar talc, on which we feel a kind of unctuosity, that prevents them

* Journal de Physique, Vol. LXIII. p. 248.

from

from being wetted. Each plane is then pressed toward the other plane by a force equal to the weight of a parallelopipedon of the fluid, the height of which is half the sum of the elevations above the level, or depressions below it, of the extreme points of contact of the interior and exterior surfaces of the fluid with the plane; and the base of which is that part of the plane included between the two horizontal lines drawn through those points. This theorem includes the true cause of the apparent attraction of bodies swimming on a fluid, when it is elevated or depressed around them. But experience shows, that bodies repel each other, when the fluid rises toward one of them, and is depressed toward the other. Having applied my analysis to these repulsions, it has led me to the following results, which I conceive may be deemed interesting by the natural philosopher and geometrician, and complete the theory of capillary attraction.

Force of this attraction.

But they are repelled, if one elevate the fluid, the other depress it.

Theory.

If we still suppose the bodies to be vertical and parallel planes, the section of the surface of the fluid included between them by another vertical plane perpendicular to these will have a point of inflexion, when the two planes are a few centimetres [a centimetre is near four lines English] from each other. If they be brought nearer together, the point of inflexion will approach nearer to that plane, toward which the fluid is depressed; if the depression of the fluid in contact with the exterior side of that plane be less than the elevation of the fluid in contact with the exterior side of the other plane. If the contrary be the case, the point of inflexion will approach the other plane. This point is always in the level of the fluid in the vessel, in which the planes are immersed. The elevation and depression of the fluid in contact with these planes are less at the interior surface than at the exterior. In this state the planes repel each other. On continuing to bring them nearer, the repulsion still subsists, as long as there is a point of inflexion. This point at length coincides with one of the planes. The repulsion still continues beyond this period; but on continuing to bring the planes nearer together, the repulsion becomes null, and is converted into attraction. At this instant the fluid is equally elevated on each side of the plane that is capable of being wetted; and it is as much elevated above the level at the interior

Circumstances under which the repulsion takes place.

At length annihilated, and becomes attraction, when the fluid is equally elevated on each side of one plane, and as much depressed on one side of the other.

interior of the other plane, as it is depressed below it at the exterior. Thus the repulsion is changed into attraction at the same moment in each plane. On bringing them still nearer, they attract each other, and proceed to unite with an accelerated motion. These planes therefore exhibit the remarkable phenomenon of an attraction at very small distances, that is changed into repulsion beyond certain limits; a phenomenon which nature presents likewise in the inflexion of light near the surfaces of bodies, and in the attractions of electricity and magnetism. There is one case, however, in which the planes repel each other, however small their distance may be; and this is where the fluid is depressed near one of them as much as it is raised near the other. Here the surface of the fluid has constantly an inflexion in the middle of the interval between them.

Case of constant repulsion

The integration of the differential equation of this surface in general depends on the rectification of conic sections, and consequently it is impossible to obtain it in finite terms. But it becomes possible, when the planes are at the distance where repulsion is changed into attraction; as this distance can then be determined in a function of the elevation and depression of the fluid at the exterior of the planes. Thus we find, that it is infinite, if the depression of the fluid on the exterior of the plane incapable of being wetted, be infinitely small: whence it follows, that the two planes never repel each other then. This may take place too even in the case where the fluid is perceptibly depressed at the exterior of the latter plane: as it is sufficient, if friction keep the fluid a little more elevated at the interior of the plane, than it would be if this friction did not exist; an effect analogous to that daily perceived in the barometer, when the quicksilver is falling. We find too by this analysis, that, if the surface of the plane capable of being wetted come to be made wet, the two planes will begin to attract each other at a very perceptible distance, greater than that at which they began to attract each other before. It is not the truth therefore to say, that two planes, one capable of being wetted, the other not, always repel each other. The same thing happens here as with two balls having the same kind of electricity, as these attract each other notwithstanding, when we vary in a suitable

The equation of the curve of the surface not obtainable in finite terms,

except within a certain distance.

Modified by friction.

When the surface is wetted it attracts at a greater distance.

suitable manner the respective intensities of their electricity, and their distance.

By means of the two following theorems, we may calculate the tendency of the planes toward each other, or their mutual repulsion. Calculation of the attraction or repulsion.

Whatever be the substances of which the planes are formed, the tendency of each of them toward the other, is equal to the weight of a parallelopipedon of the fluid, the height of which is the elevation of the extreme points of contact of the fluid with the interior plane, minus the exterior elevation; the depth half the sum of these elevations; and the breadth that of the plane in a horizontal direction. We must consider the elevation as a negative quantity, when it is changed into depression below the level. If the product of the three preceding dimensions prove negative, the tendency become repulsive. Theorem I.

When the planes are very near together, the elevation of the fluid between them is the inverse ratio of their mutual distance; and is equal to half the sum of the elevations, that would have taken place, if we suppose the first plane to be of the same substance as the second, and then the second plane to be of the same substance as the first. We must observe too, that the elevation must be put as negative, when it changes into depression. Theorem II.

We see by these theorems, that in general the repulsive force is much weaker than the attractive, which displays itself when the planes are brought very close together, and must then carry them toward each other with an accelerated motion. In this case the elevation of the fluid between the planes is very great, relatively to its elevation near the same planes exteriorly. If therefore we neglect the square of the latter elevation, with respect to the square of the former, the fluid parallelopipedon, the weight of which expresses the tendency of one of the planes toward the other, in virtue of the first of the preceding theorems, will be equal to the product of the square of the elevation of the interior fluid, by half the breadth of the plane in the horizontal direction. This elevation being, by the second theorem, reciprocal to the mutual distance of the planes; the parallelopipedon will be The repulsion weaker than the attraction.

Follows the general law of attraction.

be proportional to the horizontal breadth of the plane divided by the square of this distance. The tendency of the two planes toward each other, therefore, will follow the general law of attraction, that is to say, it will be in the inverse ratio of the square of the distance.

Put to the test of experiment,

Desirous of knowing how far these results of my theory were agreeable to nature, I requested Mr. Haüy to make some experiments on this delicate and curious point in natural philosophy.

and found to be agreeable to it.

He complied with my wishes, and found the formulæ in perfect unison with experience. He particularly ascertained the singular phenomenon of an attraction changed to repulsion by the increase of distance, as the following note I received from him will show.

Abbé Haüy's experiments on the subject.

"I suspended a small square leaf of laminar talc to a very slender thread, in such a manner, that its lower part was immersed in water. In the same water, at the distance of a few centimetres, I immersed the lower part of an ivory parallelopipedon, so that one of its faces was parallel to the leaf of talc. I then caused the parallelopipedon to advance very slowly toward the leaf of talc, keeping it still in a parallel position, and stopping at intervals, to be certain the motion, that might be imparted to the fluid, did not sensibly affect the experiment. The leaf of talc then receded from the parallelopipedon; and when, on continuing to move the latter with extreme slowness, there remained but a very small distance between the two bodies, the leaf of talc suddenly approached the parallelopipedon, and came into contact with it. I then separated the two bodies, and found the parallelopipedon wetted to a certain height above the level of the water; and on repeating the experiment, without wiping it, the attraction began sooner; sometimes indeed it took place from the first, without being preceded by any perceptible repulsion. These experiments, carefully repeated several times, always afforded the same results."

IV.

Abstract of a Memoir on the Adhesion of Bodies to the Surface of Fluids, read at the Sitting of the first Class of the Institute, November the 24th, 1806. *By Mr.* LAPLACE*.

A GREAT number of experiments have been made on the adhesion of bodies to the surface of fluids, but without any suspicion, that this adhesion was the effect of capillary action. Dr. T. Young appears to me to be the first, who made this ingenious remark †. On applying my analysis to his experiments, I have found, that it represents them as well as could be expected with regard to experiments so very delicate, and not always agreeing exactly with each other.

Dr. Young first accounted for the adhesion of bodies to the surfaces of fluids by capillary attraction.

The phenomena of capillary action being now reduced to a mathematical theory; nothing more is wanting to this interesting branch of natural philosophy, but a series of accurate experiments, in which every thing capable of affecting the result of this action is carefully removed. The want of very precise experiments is felt, in proportion as the sciences advance toward perfection. To the concurrence of the great discoveries in mechanics and mathematics with those of the telescope and pendulum, astronomy is indebted for its vast progress. We cannot therefore too strongly invite the philosopher to give the greatest precision to his operations; as we cannot sufficiently encourage the skilful artist, who devotes his labours to the improvement of the instruments of science. A single experiment badly executed, has frequently been the cause of many mistakes; while an experiment well performed subsists for ever, and sometimes becomes a source of discovery. On such an experiment we rely with confidence; but the cautious inquirer feels himself under the necessity of verifying the results given by an observer, who has not acquired a solid reputation for accuracy.

Theory of capillary attraction wants only experiments.

Cause of the progress of astronomy.

Necessity of accuracy in experiments.

* Journal de Physique, Vol. LXIII. p. 413. Nov. 1806.

† Philosophical Transactions for 1805: or Journal, Vol. XIV. p. 74, 158.

Glass on the surface of water resists separation with a force proportional to its size.

Cause of this.

When a disk of glass is applied to the surface of water standing at rest in a vessel of considerable extent, on endeavouring to separate it from the water we find a resistance proportional to the surface of the glass. On raising the glass, we raise at the same time a column of water above the level of the surface, which resembles in its figure the grooved wheel of a pulley. Its base extends indefinitely on the surface of the level: as the column proceeds upward it diminishes to about seven tenths of its height: above this it enlarges, till its summit covers the surface of the disk. To determine its volume, let us conceive in the plane of its least diameter an interior canal, at first horizontal, afterward curved vertically as far as the level surface of the fluid, and at that point resuming its horizontal direction. It is easy to perceive, that, in the case of the column being in equilibrium, the power owing to the capillariness of its surface must balance the weight of the fluid in the vertical branch of the canal. On raising the disk higher, the weight becomes more powerful from the capillary attraction, and the column separates from the disk. The weight of the column of water raised in this state of equilibrium is the measure therefore of the resistance experienced in separating the disk. If the breadth of the disk be considerable, we find by analysis, that this weight is equal to that of a cylinder of water, the base of which is equal to that of the disk, and the height the product of one millimetre [0·391 of a line] multiplied by the square root of the number of millimetres in the height to which water would rise in a tube of glass one millimetre in diameter. The surface of the water is a tangent to that of the disk; but if these two surfaces cut each other, the preceding result must be multiplied by the cosine of half the acute angle formed between them, and divided by the square root of the cosine of the entire angle.

Case where the fluid would sink in a capillary tube.

When the fluid, instead of rising, would be depressed in a capillary tube of the same materials as the disk, as mercury is in a tube of glass, the column raised by the disk has no longer the shape of a pulley: its base extends indefinitely on the surface of the fluid, but the column decreases continually from this base, till it comes into contact with the disk.

disk. The weight of this column, in a state of equilibrium, is equal to that of a fluid cylinder, the base of which is the surface of the disk, and the altitude the product of one millimetre, multiplied by the number of millimetres the fluid would sink in a tube of the same material as the disk, and of one millimetre in diameter, this product being multiplied by the line of half the acute angle that the surface of the fluid forms with the disk, and divided by the square root of the cosine of the whole angle.

All these results require a slight correction, relative to the supposition of a great diameter of the disk. I shall give this correction, which may be neglected without any sensible error for disks the diameter of which is thirty millimetres [11·74 lines] and upward.

The theory compared with experiment.

To compare the preceding results with experiment, let us consider a disk of glass 100 millimetres [3 in. 9 l.] in diameter. Mr. Haüy has observed, that in a tube of glass one millimetre in diameter, water would rise 13.569 millimetres above the level: whence it is easy to conclude, by means of the theorem above given, that the force necessary to separate the disk from the surface of water would be equal to a weight of 28·931 grammes [446·819 grs.]. Now according to Mr. Achard this force is 29·319 grammes [452·832 grs.], which differs very little from the preceding result. I made some experiments on the resistance opposed by a disk of glass applied to the surface of mercury. But to compare them with the theory, it is necessary to know the angle formed by the surface of this fluid in contact with the glass. An experiment of this kind, made with precision, is well adapted to determine this angle, which appears to be of 30° or 40°.

Two disks of glass with a stratum of water between them.

If we place two disks of glass horizontally on each other, leaving between them a very thin stratum of water, these two disks will adhere with considerable force. To determine this force, it must be observed, that the interposed fluid then takes the form of a pulley; and that the smallest radius of curvature of its surface is very nearly equal to half the thickness of the stratum. Neglecting here then, as may be done when the disks are very large, the greatest radius of curvature, we find the resistance, that the two cylinders op-

pose to their separation, equal to the weight of a cylinder of water, the base of which is the surface of the disk, and its altitude the height to which water would rise between two parallel planes of glass, as distant from each other as the interval that separates the disks. Mr. Guyton de Morveau made an experiment of this kind with two disks of glass, the diameter of which was 81·21 millimetres [3·18 inches], and he found their resistance to separation 250·6 grammes [3870·5 grs.]. According to the preceding theorem, the resistance would be only 155·78 grammes [2406 grs.]. The difference of about one third between these two results, arose, no doubt, either from the estimation of the interval that separates the disks, which requires great nicety in such small intervals; or to the inequalities of the surfaces of the disks, which it is difficult to render accurately plane.

Guyton found the resistance much greater than it should have been by theory,

from mistaking their distance,

or inequalities of their surface.

Theory of small bodies sustained on fluids.

The sustentation of small bodies on the surface of fluids depends on this general principle: "The diminution of weight of a body merging in a fluid, that sinks around it by capillary action, is the weight of a volume of fluid equal to that of the part of the body beneath the level, added to the weight of the volume of fluid displaced by capillary action. If this action raise the fluid above the level, the diminution of weight of the body is the weight of a volume of the fluid equal to the part of the body below the level, *minus* the weight of the fluid raised by capillary attraction."

Diminution of weight of bodies in fluids.

This principle embraces the known hydrostatical principle of the diminution of weight of a body plunging into a fluid: it is sufficient to omit what relates to capillary action, which totally disappears, when the body is completely in the fluid below the level of its surface.

Demonstration of this principle.

To demonstrate the principle just laid down, let us suppose a vertical tube large enough to include the body itself, and all the body of fluid that it sensibly raises, or the space it leaves empty by capillary action. Let us conceive this tube, after having penetrated into the fluid, to bend horizontally, and afterward rise vertically, preserving the same diameter throughout its whole extent. It is clear, that, in the case of an equilibrium, the weights in the two vertical branches of the tube must be equal. The weight of the

body

body therefore must compensate the vacuum it produces by capillary action; or, if it raise the fluid by this action, its inferior specific gravity must compensate the weight of the fluid raised. In the first case this action raises the body, which by this means may be retained on the surface, though specifically heavier than the fluid: in the second case it has a tendency to sink the body in the fluid. It is thus that a very slender cylinder of steel, the contact of which with water is prevented either by a varnish, or a thin stratum of air surrounding it, is supported on the surface of the fluid. If we place thus two equal and parallel cylinders, touching each other, but the extremity of one passing beyond that of the other, we perceive them immediately sliding by each other to bring their extremities on a level. The reason of this phenomenon is visible. The fluid is more depressed by the capillary action of the two cylinders at that extremity of each which is in contact with the other cylinder, than at the opposite extremity. The base of the latter extremity therefore experiences greater pressure than the other base, since the fluid around it is more elevated. Consequently each cylinder tends to unite with the other more and more: and as the accelerating forces always carry a system of bodies, the equilibrium of which is deranged, beyond the state of equilibration; the two cylinders must alternately pass each other, producing an oscillation, which, diminishing incessantly, by the resistance the cylinders experience, will at length be annihilated. The cylinders, being thus arrived at a state of rest, will have their extremities parallel. These oscillations may be determined by analysis, and we may compare the theory of capillary action on this point with experiment. These comparisons are the true touchstone of theories, which leave nothing to be wished, when by means of them we can not only foresee all the effects that must result from given circumstances, but determine their quantities with accuracy.

Steel floating on water.

Two equal pieces of thin wire, partly in contact, will become so throughout their whole length.

Theory should be tested by experiment.

If we consider the whole of the phenomena of capillary action, and their dependance on one single principle of an attraction between the molecules of bodies decreasing in a very rapid ratio, it is impossible to call this principle in question. This attraction is the cause of chemical affinities: it

Capillary attraction depends on that of the molecules of bodies, which is the cause of chemical affinities.

it does not stop at the surface of bodies, but, penetrating into them to depths which, though imperceptible to our senses, are very sensible in the action of affinities, it produces that influence of masses, the effects of which have been displayed by Mr. Berthollet in such a happy and novel manner. Combined with the figure of capillary spaces, it gives rise to an almost infinite variety of phenomena, which, like those of the celestial bodies, are now brought within the domains of analysis. Their theory is the most intimate point of contact between chemistry and natural philosophy; two sciences, which now approach each other on so many sides, that one cannot be cultivated with much success, without a thorough knowledge of the other.

and accounts for the influence of masses on them.

Allies chemistry with physics.

The resemblance of the figure of fluids raised, depressed, or rounded by capillary action, with the surfaces generated by the curves known under the name of catenary, linear, and elastic, on which mathematicians employed themselves at the origin of the infinitesimal calculus, led some philosophers to suppose, that the surfaces of fluids had a uniform tension, like elastic surfaces. Segner, who appears to have been the first that suggested this idea*, was well aware, that it could be no more than a fiction, adapted to represent the effects of an attraction between the molecules decreasing with great rapidity. This able mathematician endeavoured to demonstrate, that this attraction must have the same results: but, if we examine his reasoning, it is easy to perceive its inaccuracy; and we may conclude from the note appended to his researches, that he seems not to have been satisfied with it himself. Other philosophers, resuming the idea of a uniform tension of fluid surfaces, have applied it to various capillary phenomena. But they have not been more successful than Segner, in the explanation of this force; and the most able have contented themselves with considering it as a means of representing the phenomena. In giving into all the conjectures, which may arise from the first view of these phenomena, we may hit on some truths: but they will almost always be mingled with many errours, and the discovery of them belongs only to him, who, separating

Capillary attraction produces a catenary curve,

but the surfaces of fluids have not a uniform tension.

Conjecture may hit upon truths,

but he is the discoverer who observes or in-

* Mem. of the Royal Society of Gottingen, Vol. I.

them

them from this mixture, goes so far as to establish them on solid foundations by observation or mathematical investigation.

vestigates them with accuracy.

V.

Account of a Loom to be worked by Steam or Water; by Mr.* JOHN AUSTIN, *of Glasgow.*

SIR,

AFTER much trouble, expense, and reiterated experiments, I have happily succeeded in completing a new WEAVING-LOOM, a Working-Model of which, with cloth in it, is presented to the Society for their inspection. It has, upon trial, succeeded beyond expectation, answers in every respect the purpose for which it is intended, and has met with the approbation of manufacturers of the first respectability in the country.

New looom for weaving.

After many different attempts, I think that I have brought my weaving-loom, which may be driven by water, or steam, to such a state of perfection, as to prove its utility, the more it is known and employed.

Worked by water or steam

My first attempt was made in the year 1789: I at that time entered a caveat for a patent, but relinquished the idea of obtaining one, and have since made many improvements upon my original plan. In 1796. a report in its favour was made by the Chamber of Commerce and Manufactures at Glasgow; and in the year 1798, a loom was actually set at work, at Mr. J. Monteith's spinning-works, at Pollockshaws, four miles from Glasgow, which answered the purpose so well, that a building was erected by Mr. Monteith, for containing thirty looms, and afterwards another to hold about two hundred.

attempted long ago, and carried into execution.

The model now submitted for inspection is an improvement upon those constructed for Mr. Monteith.

* Transactions of the Society of Arts, 1806. The gold medal of the Society was voted to MR. AUSTIN for this invention.

The

Enumeration of its advantages.

The following are the advantages which my Loom possesses.

1. That from 300 to 400 of these looms may be worked by one water-wheel, or steam-engine, all of which will weave cloth, superior to what is done in the common way.

2. That they will go at the rate of sixty shoots in a minute, or two yards of a nine hundred web in an hour.

3. That they will keep regular time in working, stop and begin again, as quick as a stop watch.

4. They will keep constantly going, except at the time of shifting two shuttles, when the weft on the pirns is done.

5. In general, no knots need to be tied, and never more than one, in place of two, which are requisite, in the common way, when a thread breaks.

6. In case the shuttle stops in the shed, the lay will not come forward, and the loom will instantly stop working.

7. They will weave proportionally slower, or quicker, according to the breadth and quality of the web, which may be the broadest now made.

8. They may be mounted with a harness, or spot heddles, to weave any pattern, twilled, striped, &c.

9. There is but one close shed, the same in both breadths, and the strain of the working has no effect on the yarn behind the rods.

10. The bore and temples always keep the same proper distance.

11. There is no time lost in looming, or cutting out the cloth; but it is done while the loom is working, after the first time.

12. The weft is well-stretched, and exactly even to the fabric required.

13. Every piece of cloth is measured to a straw's breadth, and marked where to be cut, at any given length.

14. The loom will work backwards, in case of any accident, or of one or more shoots missing.

15. Every thread is as regular on the yarn beam as in the cloth, having no more than two threads in the runner.

16. If a thread should appear too coarse or fine in the web, it can be changed, or any stripe altered at pleasure.

17.

17. They will weave the finest yarn, more tenderly, and regularly, than any weaver can do with his hands and feet.

18. When a thread either of warp or weft breaks in it, the loom will instantly stop, without stopping any other loom, and will give warning by the ringing of a bell.

19. A loom of this kind occupies only the same space as a common loom; the expense of it will be about half more; but this additional expense is more than compensated by the various additional machinery, employed for preparing the yarn for the common loom, and which my loom renders entirely unnecessary.

20. The reeling, winding, warping, beaming, looming, combing, dressing, fanning, greasing, drawing bores, shifting heddles, rods, and temples, which is nearly one half of the weaver's work, together with the general waste accompanying them, which is about six per cent of the value of the yarn, and all which occur in the operations of the common loom, do not happen with my loom, which, by its single motion, without further trouble, performs every operation after the spinning, till the making of the cloth is accomplished; by which, independent of the saving of the waste, the expense incurred for reeling, warping, winding, &c. is saved, amounting to above twenty per cent of the yarn.

21. The heddles, reed, and brushes, will wear longer than usual, from the regularity of their motion.

22. More than one half of workmanship will be saved: one weaver and a boy being quite sufficient to manage five looms of coarse work, and three or four in fine work.

These advantages, which from experience my weaving-loom has been found to possess, and which upon inspection will be perceived, will, I presume, be esteemed of some magnitude.

My loom, as now constructed and improved, is much simplified, so that the manual labour requisite is trifling; and if it is encouraged by the Society of Arts, I am sensible much advantage will arise from their approbation, and the publicity it will in consequence receive.

I am, Sir,

Your humble servant,

JOHN AUSTIN.

Certificate

Certificates of its utility.

Certificates were produced from Messrs. Hugh Cross, Matthew Persten, and David Mutrie, dated Glasgow, October 12, 1796, stating that, by appointment of the Chamber of Commerce in Glasgow, they had inspected the Loom, constructed by Mr. Austin, and were of opinion, that it will be found to contain some ingenious and useful improvements, by producing saving and facility in several of the ordinary operations.

Messrs. Neil, Macvicar, and Thomas Henderson, of Edinburgh, certified on the 12th of April, 1804, that they had seen, in the Trustees office there, the model of Mr. Austin's Loom, and that they thought it ingenious, and the best they had then seen.

Further Certificate, from Edinburgh, dated April 14, 1804, from Mr. John Drummond, and from Messrs. James Reid and John Waugh, partners in the house of Walter Bigger and Co. linen-manufacturers, testify to the ingenuity of Mr. Austin's Loom, and that it is capable of being employed to the great advantage of the manufactures of this country.

Mr. Austin having left a complete Working-Model of his Loom with the Society of Arts, &c. a reference to it will convey an idea of its principles, better than any description that might be attempted; as from the variety of minute parts in it, the Committee of the Society have thought it impossible to have a drawing of it, upon their usual scale, which can be rendered sufficiently intelligible.

VI.

Observations and Experiments respecting the Art of making Copies of Written Paper by Pressure. By R. T.

SIR,

Copying machine.

A Few years ago, a Machine, called a Copying Machine, was offered to the Public for the purpose of obtaining a copy from any recently written paper.

To merchants and others, who are in the habit of writing a great number of letters, &c. of which they wish to have a copy

copy, this invention has been of so great utility, that it has now come into very general use.

The method of using this instrument, which is a rolling press, is briefly this: Having covered the paper to be copied with a piece of damp copying paper (a kind of white, thin, unsized paper, made on purpose), place it between oiled papers on a board, cover it with some blotting paper, and pass it through the press: a copy, which is legible through the copying paper, is thus obtained from writing that has been written only a few hours. Thus far this machine fully answers the purpose; but when old writing is made to undergo this process, no effect is produced.

Method of using it.

Does not answer for old writing.

Should any method be discovered of obtaining copies from old writing, it would prove a valuable acquisition to many persons, and to me in particular. It is with the view of obtaining information on this subject, that I have troubled you with this letter; you, or some of your correspondents will, I hope, through the medium of your valuable Journal, favour the public with some communication, that may throw a light on this interesting subject.

Information on this head desired.

The following account of a few experiments I have made will, perhaps, be of service in forming a judgment as to the means most likely to succeed; or they may be useful to any one, who may choose to prosecute the matter experimentally.

I remain, Sir,

Your obedient servant,

To Mr. NICHOLSON. R. T.

Experiments.

I first tried the most violent pressure (both with and without the substances hereafter mentioned), without advantage. A moderate pressure is best. Writing can seldom be got out after it has been written more than 24 hours.

Moderate pressure best.

In taking off writing, a considerable improvement was discovered; it consists in covering the copying paper with flannel instead of oiled paper. By glueing the copying paper on a piece of white paper, the writing is rendered more legible. Notwithstanding these, and using boiling water instead of cold, the old writing continued refractory.

Mechanical

Solutions applied to the writing, or the copying paper

Mechanical means having failed, it was necessary to encrease the power by the assistance of chemistry.

With this intent, I soaked either the old writing or the copying paper in various solutions, and passed them through the press. Old writing is rendered blacker by being soaked for some hours in infusion of galls, but it has no power to bring it on to the copying paper.

Infusion of galls.

Green vitriol.

Solution of sulphat of iron produced no effect.

Prusiate of potash.

With triple prussiate of potash a faint copy was sometimes obtained; often it had no effect: a few drops of sulphuric acid added to it increased its power, but the whole was rendered green.

Hidrosulphuret of ammonia has more powerful action on writing than any thing I have yet tried. When this liquid is poured on faded or almost any kind of writing, it changes it to an intense black colour. By the aid of this preparation, I have been enabled sometimes to procure tolerably good copies, but could not obtain a constant effect, though I often varied the progress.

Hidrosulphuret of ammonia.

But the action of hidrosulphuret of ammonia is incomplete, the black it gives to writing is not permanent, and on some writing it has no effect.

As this substance appeared more likely to succeed than any other, I was induced to examine it more particularly, but the result has convinced me its power is inadequate to the purpose.

The reason it gives a black colour to writing is this: almost all inks contain an excess of sulphate of iron; the ammonia combines with the acid, and the sulphuretted hidrogen with the iron, forming the black colour; and because different inks contain different proportions of sulphate of iron, they will not be equally affected by the hidrosulphuret of ammonia.

Hidrosulphuret of iron decomposed by the air.

The hidrosulphuret of iron is decomposed by the carbonic acid of the atmosphere.

The following experiments will prove these positions. Write on paper with;

No. 1. A solution of tan.

No. 2. A solution of green sulphate of iron.

No.

No. 3. A pale ink formed with green* sulphate of iron and solution of tan, having excess of sulphate.

No. 4. An ink as above with excess of tan.

Pour on the writings hidrosulphuret of ammonia; Nos. 1 and 4 will remain unchanged, Nos. 2 and 3 will instantly become intensely black, but No. 2 changes by exposure to a rusty brown, and No. 3 becomes faint.

If hidrosulphuret of ammonia is poured into solution of sulphate of iron, a black powder precipitates; when this is exposed to the air it turns to a red rust.

Although the hidrosulphuret appears to have no effect on dry tannate of iron, yet when poured into the ink No. 4, it changes it to a red colour; writing written with this mixture becomes nearly black in an hour. When filtered, a red substance remains, and the filtered liquor is of the same colour.

Ink not simply a black powder suspended in water.

I have somewhere seen it asserted, that ink consists of a black powder suspended in water, so extremely fine as to pass with the liquor through a paper filter; this is not exactly the case.

If ink, prepared as No. 3, be exposed to the air a short time and filtered, a black mass remains on the filter, and the liquor that passes through is of a fine deep blue colour: if a drop be let fall from the filter on a piece of ivory, and examined immediately, it will appear a homogeneous liquor, but in the course of a minute numerous black particles will be seen floating in it.

These effects are best perceived with a glass. Some of the filtered ink placed in a wine glass is speedily covered with a film; on shaking the glass, black pieces will be seen in the apparently colourless liquid, that trickles down the sides.

A soluble blue and insoluble black substance;

From this it is evident, that new ink consists of at least two substances, one soluble in water, and communicating to it a dark blue colour, the other an insoluble black powder.

* This was the green vitriol of commerce boiled on iron filings to deprive it of any excess of acid, and to bring it to a minimum of oxigen; but I do not know whether it was exactly in that state. The ink is of a blue colour, and passes through the filter without leaving scarce any residuum: the writing written with it is at first excessively pale, but gradually becomes black.

It

both probably tannates of iron.

It is highly probable, that these are tannates of iron, differing merely in the proportion of oxigen which they contain, especially as the blue is changed into the black by exposure to the air.

Ink not well understood.

The nature of ink is at present not well understood; but it is not my intention to undertake its investigation, I leave that task to an abler hand. I beg leave, Sir, to conclude, by proposing a few queries for your consideration, and for that of your correspondents.

Queries respecting its nature.

In what do new and old writings differ?

Is the difference in consequence of the particles of the old writing having become more firmly united together by time; or is it on account of their having undergone some chemical change*? If the latter, in what does this change consist?.

Is it the tannate ofiron, which has suffered an alteration? or is it the gum, which all inks contain?

Is it there any substance capable of dissolving, without decomposition, the black tannate of iron†?

VII.

Of Violet Purple, and the different Tints that may be derived from it; by John Michael Haussman‡.

Water not the only menstruum ofdyes.

WATER is not the sole menstruum capable of extracting the colouring parts of plants, in order to enable them to adhere to alumine or oxide of iron fixed in any cloth. There are vegetables, as alkanet root, which give out their colouring

* That it is not on account of a chemical action having taken place between the tan and the gelatine of the paper, will appear from this, that unsized paper yields a copy no easier than any other.

† From the experiments of Bouillon Lagrange, which render it probable, that strictly there is no such thing as gallic acid, and from the manner in which ink is generally prepared, viz. by long boiling, which must dissipate the acid if it exist, I have been induced to omit taking it into account.

‡ Annales de Chimie, vol. lx. p. 288, December, 1806.

matter

matter only to alcohol. I shall not attempt to define the nature of the colouring matter of alkanet: it is so readily decomposed by the continued action of heat, even below the temperature of boiling water, that, after it has been extracted by alcohol, it cannot be concentrated by evaporation without being destroyed; so that it is impossible to make any farther use of the spiritous part of the tincture of alkanet, as I have convinced myself, by reducing a certain quantity to one fourth by distillation. The alcohol that came over appeared to me perfectly pure; and the residuum was muddy, and unfit for dyeing. I confess I was to blame for not having examined it more thoroughly, to see whether it contained any thing oily or resinous; but I had then no other object in view, than to avail myself of the colouring properties of alkanet, with which I had reason to be satisfied.

Its colour decomposed at a low heat.

On mixing a sufficient quantity of spirituous tincture of alkanet with six or eight parts of pure water in a copper boiler; and afterward dyeing in it hanks of cotton prepared for Adrianople red, according to my process inserted in the Annals de Chimie, year 10, by Mr. CHAPTAL, at that time Minister of the Home Department; at the expiration of an hour, raising the fire gradually till the bath was brought to boil, they were of a fine violet purple colour. To produce this colour constantly of the greatest brightness, the cotton must not be made dull by the preliminary preparations, and consequently must not be galled. The linsed oil I employed for the preparation was boiled with ceruse, taking care not to burn it, that it might not soil the cotton.

Tincture of Alkanet, with 6 or 8 parts of water.

Gave cotton, prepared for Adrianople red,

a fine purple,

The great lustre of this violet purple on cotton, which surpasses that of the finest satin dyed in the common manner, suggested to me the idea of producing it in fine printed goods. My expectations were so far answered with success, that we presently manufactured some whole pieces of long shawls, with a ground of this colour, for Mr Soehné, Sen. and Co. of Paris, who received them a few years ago, and admired them very much. They found the price, however, too high for the times. Formerly, when it was common for ladies of fashion to wear printed calicoes both in summer and winter, it was necessary for those who would force a business to have arti-

of a lustre superior to that of satin,

but too expensive for the present times.

cles

cles of this kind of very high price. I had proofs of this two and thirty years ago, when I lived at Rouen; for having then a few gown pieces of ten ells, of a very rich pattern, to sell on commission, I disposed of them without difficulty at two and thirty louis a piece. These articles were from the manufactory of the illustrious John Henry von Schule, of Augsburg, who is well and justly entitled to be styled illustrious, as the first manufacturer in Europe, who carried the printing of calicoes to great perfection and extreme beauty. His articles have made so much noise in all parts of the mercantile world, that the emperor of China desired to see them, and admired them in comparison with the productions of his own dominions.

In 1775, rich prints sold at Rouen for £1 13 6 a yard.

Mr. von Schule the first manufacturer of printed goods in Europe.

Cottons intended to be printed with violet purple grounds, and to have any white figures, require to be very well bleached, that they may be muddied as little as possible in dyeing: for, though the violet purple is such a fixed colour, as to support the action of the alcaline lixivium of oxigenized muriate of potash, without being much weakened, the white is restored but slowly.

Cottons must be well bleached.

Alumine fixed in the cloth, and saturated with the colouring particles of tincture of alkanet, will still admit the colouring matter of other vegetable or animal substances; which gives rise to an infinite number of other tints, that may be increased indefinitely, by more or less diluting the acetate of alumine employed in the printing; and by dipping the violet purples, and their derivative tints, thus produced, in a bath of madder, cochineal, kermes, brazil, weld, quercitron, &c. By mixing these drugs in different proportions, the tints may be greatly increased in number; and still farther by mixing more or less acetate of iron with the concentrated or diluted solution of acetate of alumine.

Other colours may be dyed upon the alkanet, so as to vary the hues.

Cotton printed with oxide of iron, or a concentrated solution of acetate of iron, takes a greenish black from the tincture of alkanet: and by diluting the solution of acetate of iron in different proportions, we shall obtain a great variety of grays, more or less deep, and more or less green. These tints are equally susceptible of variation by means of the dyeing drugs already mentioned.

Alkanet with oxide of iron.

If we wish to produce other colours by the side of the ground

Other colours printed by the side of it.

ground of purple violet, or its derivative tints, without perceptibly altering this ground, it is necessary, before the blocks with other mordants are applied, to pass the alkanet ground through dilute sulphuric acid, to carry off the alumine, that has been left untouched by the colouring particles of the alkanet. The purple and its derivative tints will be reddened a little indeed by the action of the acid, without however being much weakened.

Linen prepared in the same manner as cotton presents nearly the same colours and tints when dyed with tincture of alkanet; and admits the same variations by means of other colouring drugs, or the acetate of iron. Linen may be dyed with alkanet,

The same may be said of silk properly alumed. It affords very brilliant colours by being passed through tincture of alkanet; which however only gives the silk a muddy tinge, if it be prepared with a solution of tin of any kind, instead of being alumed. This shows the little affinity of the oxide of tin for the colouring particles of alkanet, which produce no better effect on linen or cotton, prepared with solutions of the salts of tin. and silk, but not with solution of tin.

The same inconvenience would probably take place with wool, which I have not treated with tincture of alkanet: but no doubt it would exhibit nearly the same colours as cotton, linen, or silk, after having been well alumed. Woollen.

VIII.

On Cast Iron; *by* Professor Proust.*

GRAY and black cast iron afford an aromatic hidrogen, which appears to me to hold in solution a part of the oil that is formed during their solution in acids. This hidrogen burns heavily; and its flame is tinged with yellow and green. Four inches of this gas, however, burned with eight of oxigen, consumed only two, or no more than pure hidrogen would have done. The residuum did not render lime-water turbid. I suspected, therefore, that the oily particles might Aromatic hidrogen from gray and black cast iron.

* Journal de Phisique, vol lxiii. p. 463, December, 1806.

have escaped combustion; but we must not forget, that very small quantities of carbonate of lime are soluble in lime-water.

Six of this and 16 of oxigenized muriatic gas reduced by mixture to ·5

Six inches of this gas, and sixteen of oxigenized muriatic gas were reduced in the space of an hour to half an inch, the greater part of which was still oxigenized muriatic gas. A cloud was formed at the instant of mixture, and a light greasy pellicle floated on the surface of the water, but I was not able to examine it. This gas likewise contains phosphorus. Phosphorus, in fact, must occur in cast iron oftener than is imagined; for I have perceived a phosphate in almost all the solutions of our Spanish cast iron. But beside the ore there are some kinds of charcoal that contribute to this. That of the evergreen oak, for instance, must contain either phosphorus or a phosphate, since the latter is found in its ashes.

Greasy pellicle formed.

Contains phosphorus.

Phosphate in the ashes of the quercus ilex.

Their Plumbago.

Carbon separated from cast iron appears like plumbago.

Is plumbago a carburet?

The carbon separated from cast iron has the leaden appearance, lustre, and scaly texture of plumbago; particularly when it has been thoroughly freed from iron by the muriatic acid: but is plumbago in fact a combination of iron with carbon, a metallic carburet, as it has been considered ever since the time of Scheele? His own experiments, in conjunction with some particular facts, lead me to doubt this; and I am at present fully persuaded, that, before we give implicit credit to this combination, it would be proper to subject it to a fresh examination.

Supercarburetted Cast Iron.

Cast iron overloaded with carbon by long fusion.

I had occasion to examine some cast iron that had been refined according to Grignon's principles, or by keeping it a long time in fusion. The cannons made of it were proved by the corps of artillery under the reign of Charles III, and would not stand the trials.

Its appearance and properties.

This iron, when broken, had not the granulous appearance of gray cast iron: it exhibited to the eye a heap of small needly cones, very obtuse, between which micaceous scales of plumbago were visible when inspected with a lens. The superabundance of this facilitated the crystallization. Under the hammer it is compressed, and crumbles. The file cuts it

it very easily. A skilful workman succeeded in forging a piece without melting it, and formed a plate of it, which, after tempering, appeared to be very steely. Hence I conceive it follows, that, if cast iron gain in metallization by continuing the heat, it loses by the diminution of its oxide a principle, that seems indispensable to the solidity of its texture.

Tough cast iron, a solution of oxide of iron in the metal.

If this oxide, which serves the purpose of interlacing the metallized parts, and of preserving a more complete contiguity between them, happen to be deficient, the liquidity of the fused mass cannot avoid being diminished, and its place must be supplied by carbon, to keep up this effect. But when the iron owes it liquidity to this new principle, it is far from having the same coherence or tenacity as in the former case. Whatever may be thought of this opinion by those metallurgists, who are engaged in casting artillery, I conceive it will be of use to them, to preserve the history of these facts.

If a carburet, still a compound united with an excess of one of its elements.

But if we continue for the present to consider the carburet of iron as an actual combination, we must allow, that its existence, or its solution in cast iron, affords us an example of a combination, a compound with the excess of one of its elements, or, if you please, of another kind of union, to which Mr. Berthollet does not appear to me to give a full assent.

Cast iron imperfectly reduced by pit coal.

I have examined cast iron obtained with the pit-coal of Asturia in furnaces, that had neither the height, nor strength of blast, commonly required to reduce iron ore by this combustible. This cast iron, on coming out of the crucible, boiled till it began to fix. The result was white, blistered masses, fit for nothing but making cannon balls. It was easy to see, that this ebullition was nothing but a continuation of the effervescence, which had not terminated in the furnace.

Afforded less hidrogen.

Its solution confirmed this opinion, for it afforded infinitely less hidrogen than white cast iron.

Cast iron retains oxide in solution.

The labours of Bergman, Berthollet, and many other scientific men, confirmed by the methods practised in England, to promote the disoxidation of the parts in which this process has not taken place, scarcely admit of a doubt, that cast iron is nothing but metallic iron serving as a menstruum to a portion of its oxide. But are not such solutions so many examples of compounds dissolved in an excess of one or other of their elements?

Analogous instances.

Sulphurate of copper dissolves in copper, one of its elements. The black coppers contain it; and even sulphurate of iron and sulphurate of silver likewise. We may presume from these instances, therefore, that there would be nothing extraordinary in finding sulphurets, phosphurets, and carburets, dissolved in their respective metals; and consequently, to see these metals dissolve other oxides. If cast iron be an instance of this, that of the oxide of copper in its metal is another, for which we are indebted to Mr. Chenevix; and the experiment of Fernandez on the solution of muriate of silver in its metal is a third.

Copper.

Silver.

Superphosphurretted carbon.

Phosphuret of carbon is a compound, that dissolves in phosphorus, one of its elements, in we know not what proportions.

Amalgams.

Amalgams are compounds, some of which appear to be proportional, and others not. Some separate from the excess of mercury, and afford means of studying them: others remain in complete solution in it in progressive quantities, the extent of which is not known.

Water.

Water is a compound, which by the assistance of circumstances dissolves in oxigen gas, and in hidrogen gas, or in either of its elements.

Hidrogen.

Hidrogen is an element of fat oils, volatile oils, camphor, &c.; but we find, that, during their passage in vapour through a red hot gun-barrel, hidrogen can disengage itself from the coal, and dissolve a part of these vapours.

Hidrurets.

Nothing surely can be objected to our considering, by an extension of these principles, solutions of sulphur, phosphorus, carbon, arsenic, zinc, &c. in hidrogen, not as simple solutions without any measure, but as so many compounds in due proportion, as so many hidrurets of sulphur, phosphorus, &c. which an excess of the solvent may hold in solution.

Various compound solutions in chemistry.

If we cast our eye over the whole field of chemical science, we shall discover there too a multitude of compounds, which dissolve others; some in proportions that are easily estimated by a separation of the excess; and others, that, not yielding to this method, continue to fluctuate in the ocean of indeterminate quantities: so that even at the present hour we are ignorant, whether we ought to place in the same line

the

the compounds that are confined to constant proportions, and those that are subject to none, though both are the result of the same power.

Mr. Berthollet, in his Third Series of Inquiries concerning Affinities, expresses himself thus:

Objection of Berthollet.

"Proust asserts, that compounds, the proportions of which are fixed, may unite with an excess of one of their elements in an indefinite proportion; without defining the characters, that distinguish combination from this other kind of union. It is obvious, that, in consequence of the latter distinction, it would be difficult to object to him any observation, which he would not find means to explain."

Proust's answer.

If the preceding facts, to which many others might be added, since the works of the elder chymists are loaded with them, sufficiently prove the existence of these kinds of union, or solutions of compounds by their elements, or even by other compounds; it would appear to me superfluous to insist longer upon them: but I have been able to make them concur in the explanation of certain phenomena, without any contradiction of principles. As to the characters that distinguish them, or ally them to those compounds that range under the laws of proportion, I am entirely of Mr. Berthollet's opinion. But how should I define those characters? All the elements of such unions are not sufficiently known. Chemistry not having yet called for their being subjected to a particular study, it is enough for the present to exhibit them as incontestable facts, till reflection determines their proper place in the edifice of science.

Hidrate of Iron.

A fine yellow ochre, containing a little carbonate of lead.

Mr. William Talaker, our collector for the Cabinet of Madrid, found a very fine yellow ochre in the mountains of Artana, in the kingdom of Valentia. It contains a little carbonate of lead, though there is no mine of that metal in the neighbourhood. This was taken up by weak nitric acid, without altering the colour of the mineral.

On distillation gave out 12 of water.

This ochre, freed from lead and carefully dried, was subjected to distillation in a retort of ten inches capacity. The aqueous vapour that arose completely expelled the air from the retort, and with it about half an inch of carbonic acid

gas

gas. A hundred parts of the ochre freed from lead were reduced to eighty-eight of a pretty fine red powder. The product was pure water.

Muriatic acid separated ·44 of sand.

Muriatic acid applied to the residuum, separated forty-four parts of sand: consequently there were forty-four parts of red oxide likewise.

Hidrate of red oxide.

If forty-four parts of this oxide were combined with twelve of water, one hundred parts must have been united with twenty-seven in this oxide. It was therefore a hidrate, with base of red oxide.

If the red oxide, which is generally less disposed to enter into combination, be capable of producing a hidrate, must not the black oxide be much more so? The hidrate of iron then with base of oxide at a minimum will be found some day, either pure, or in a compound of this metal. I am of opinion it is in this state, that it makes a part of the carbonate of iron, the base of which is always at a minimum.

Hidrate of black oxide in carbonate of iron.

IX.

On Filtering Stones, and the Method of determining the specific Gravity of Substances with large Pores. By Mr. GUYTON *.

Filtering stones supposed to be arenaceous.

BOTH Linneus and Wallerius have spoken of a filtering sandstone: *cos filtrum particulis arenaceis æqualibus, aquam transmittendo stillans: cos particulis arenaceis puris aquam transmittens.* On their authority most mineralogists have classed these stones among the varieties of arenaceous quartz: but we do not find, that they had ascertained whether the silex in them were pure, or merely the predominant principle. The former appears however to be the most general opinion, since, excepting one passage in Kirwan, where he mentions the *pierre de liais* among the silicicalcareous stones as porous, and used for filtering †, we do not find in the

Kirwan mentions one partly calcareous.

* Annales de Chimie, Vol. LX, p 121. Nov. 1806.

† Elements of Mineralogy, Vol. I. p. 102.

most

most modern works on mineralogy any mention of that kind of stone, which is so much used at Paris for filtering water, and the advantages of which have been confirmed by an experience of more than thirty years.

Common filtering stone of Paris.

This is a yellowish free stone, of a middle-sized grain, soft enough to be cut with a toothed saw, easily admitting its grain to be rubbed out by the fingers, and yielding a fine powder, by rubbing two pieces against each other.

Spec grav.

I found its specific gravity to be 2·322. A piece, weighing while dry 102·155 grammes, weighed 114·5, after it had lain ten minutes in water, though it was carefully wiped; which gives an increase of 12·545 grammes, or very near an eighth of its weight.

Effervesced with acids.

A hundred decigrammes of this stone dissolved slowly in diluted nitric acid; and the carbonic acid gas evolved occasioned a diminution of weight of 33·59, including the small quantity of water, which it always carries off with it.

The filtered solution left only 12·11 of siliceous earth.

The lime precipitated by sulphate of pot-ash gave 139 of sulphate of lime.

Hence we may deduce the composition of this stone, consisting of

Its component parts.

Carbonate of lime	87·89
Silex	12·11
	100

Place where found kept secret.

I was desirous of knowing the place where this stone was found in strata of sufficient extent, to supply the shops that work it up into filters for water; but from all my inquiries, and what information I could get, it appeared, that the inventor of these filtering stones, who has thus rendered a real service to society, has thought proper to keep the knowledge to himself.

Many similar stones used in building.

In consulting the description given by Mr. Brisson, however, in his Treatise on Specific Gravity, of the stones used for building in Paris and its environs, from the collections of Perronet and Wailly, several are found to exhibit the same characters so completely, that we cannot doubt their possessing

possessing the same properties, and being applicable to the same uses.

There are ten in particular, that resemble it in want of hardness, and size of grain, and that are capable of receiving into their pores from eight to twenty-five hundredths of water: such are, among others, those from Maillet quarries, at St. Leu, and from the quarries at Vergelet, Gentilly, St. Germain, Conflans, St. Honorine, and Bouré, near Montrichard.

The same author mentions, in the series of sand stones, under the name of *filtering sand stone*, a piece from a stone used as a filter, that absorbed a tenth of its weight of water; while the crystallized siliciferous carbonate of lime of Fontainebleau did not absorb quite four thousandth parts; and among the true sand stones, such as those used by paviors, cutlers, &c. and even those in which the remains of organized bodies sometimes occur, there are none that admit so much. These circumstances lead to the supposition, that the specimen subjected to this trial by Mr. Brisson actually belonged to a filter of the same kind as those now generally used; and that he gave it the name of sand stone, merely from the preconceived notion, that the property of filtering existed only in stones of this species.

True sand stones absorb much less water.

We may conclude then, that the filtering stone employed for domestic purposes at Paris is not a sand stone, but a carbonate of lime, containing only 12 or 13 per cent of silex, in such a state of aggregation, as to leave pores sufficiently open to admit water to run out of them gradually as they imbibe it; that it differs not only from the sand stones with siliceous cement, but likewise from the argillaceous sand stones, such as the grindstones of Geneva, Brives, &c. which in time imbibe a pretty considerable quantity of water, but let it pass through with much more difficulty: and that several of the quarries I have pointed out from Mr. Brisson may have calcareo-siliceous strata of the same nature, and possessing the same property.

The proper filtering stone a carbonate of lime, with ·12 or ·13 of silex.

To remove all doubts on this head, it appears to me necessary to offer some remarks on the mode of determining the specific gravity of substances with large pores.

Errours in determining the

It may appear surprising, that I assign to the stone I have described

described and analysed, a specific gravity of 2·322; while Mr. Brisson gives no more than 1·232 for that of the piece of filtering stone, which I have mentioned as serving to establish a similarity. But it must be considered, that, to obtain this result, Mr. Brisson *adds* to the weight necessary to restore the equilibrium, when the substance is immersed in water, the *weight of the quantity of water that has penetrated it.* Such was the method adopted by the author, for substances capable of imbibing water, which appears to me to require a farther examination, though followed by many natural philosophers. It is true Mr. Brisson gives to the specific gravities of these same substances a second expression, derived from a calculation, in which the absolute weight, or the weight taken in air, is increased by the weight of the water absorbed. But neither of these expressions can give the true ratio of the mass of matter to the actual place it occupies; since in the first, that water is reckoned as displaced, which only succeeds to the air that before occupied the pores, and which ascends in bubbles from every part of the surface; and in the second, the weight of the mass is confounded with that of the fluid employed to circumscribe its solid parts.

specific gravity of porous substances, from supposed corrections.

In fact, if I had proceeded on this principle, I should have found the specific gravity of the filtering stone no more than 1·813, which comes very near to that assigned by Mr. Brisson. On the other hand, if we apply to the data of his experiment the simple calculation of dividing the weight of the body in air by the weight necessary to add to restore the equilibrium when it was weighed in water, we shall have as the quotient 2·391, consequently still a little more than the same calculation for the stone I examined gave me.

But no correction necessary, when the substance is sufficiently permeable.

Sand stones and filtering stones are not the only fossil substances, that receive into their pores the surrounding medium. Chalcedonies, pitchstones, steatites, asbestos, mesotype, schists*, some micas, and even, according to Gerhard, some varieties of jade, are more or less penetrable by water.

Other fossils penetrable by water.

* Mr. Ludicke has described some hard schists, which he found to imbibe and part with moisture so regularly, that they were capable of answering the purposes of an hygrometer.

This

Spec gravity sought by the naturalist, as illustrating the nature of bodies.

This property unquestionably ought to be noticed in describing them: it makes a part of the characters, the aid of which is requisite to the naturalist in discriminating species; but when he seeks the true specific gravity of any substance, it is to acquire a more intimate knowledge of its nature, not to derive from the measure of a surface full of pores, and roughened with asperities, a gross calculation of the solidity of its mass, as if his object were the estimation of a load.

The problem, which it is of real importance to the progress of science to solve, is to determine the exact ratio, that the proper substance of the body under examination bears to the bulk of its contiguous parts, that leave no more spaces into which the surrounding fluid can have access. The water, which is absorbed as the air escapes, can no more be considered as water displaced by the solid, than that imbibed by a sponge; and we should fall into a great error, if we were to estimate its density on this principle. It would be superfluous to say, that in all cases we suppose the water to have no chemical action, as it has on salts; for then the hydrostatic balance could not even give an approximation to the truth, and we must have recourse to Say's stereometer*; or if we have not this ingenious instrument, which is not yet in very general use, we must employ a fluid that has no action on the subject to be examined, as for instance, water completely saturated with the same salt. Thus I used a saturated solution of nitrate of potash, when I was engaged in the year 11, as member of the committee appointed by the minister at war, to give a comparative table of the specific gravities of all the different kinds of gunpowder used in the fleets or armies of various nations.

Spec. grav. of soluble substance, found by Say's stereometer;

or by weighing in a fluid that will not dissolve, or is already saturated with them: as gunpowder in solution of nitre.

The same principles led me to suspect, a few years ago, the errour, into which most mineralogists had fallen, in ascribing to pumice stone a specific gravity even inferior to that of water. Mr. Klaproth observes, in his analysis of that of Lipari, that, though it contains more than 0·17 of alumine, it is not at all attacked by acids: this, added to the hardness we find in its smallest particles, though they are easily separable, indicate a state of combination inconsistent

Errour in regard to pumice stone.

* See a description of it, Annales de Chimie, Vol. XXIII. p. 5.

with

with the idea of rarefaction attached to such lightness. It is evidently owing therefore to the multitude of pores and hollow spaces, into which the water cannot penetrate, to circumscribe the volume of the solid parts. Powdered pumice stone afforded me a specific gravity of 2·142*; and this is the only method of weighing hydrostatically porous bodies, so as to obtain a constant expression of their density, truly comparable, and affording a just idea of the power of aggregation possessed by their integrant parts, which is the most important point, on which any light can be thrown by a comparison of specific gravities.

Its spec. grav. 2 142.

Porous substances should be weighed in powder.

X.

Report on a Sculptured Head of Flint, with a Covering of Calcedony, made to the Physical and Mathematical Class of the Institute, March 31, 1806. *By Mr.* GUYTON †.

MR. MILLIN, our associate, of the class of history and ancient literature, having had an opportunity of examining a piece of sculpture found in the Faubourg du Roule, thought it his duty to offer it to the inspection of the physical and mathematical class, as an object leading to questions that were interesting both to mineralogy and the arts; and you have commissioned Messrs. Berthollet, Vauquelin, and myself, to make a report to you on the subject.

Antique submitted to the Institute for examining.

Committee.

The fragment was very obligingly entrusted to us by Mr. Cerf, to whom it belongs. It was found, four months ago, in the garden of a house, that was formerly part of the

Where found.

* See Annales de Chimie, Vol. XXIV. p. 204.

N. B. I did not neglect this method of verifying the specific gravity of the filtering stone. I reduced it to a fine powder; and the moment it was immersed in water, all the air interposed between its parts, or rather that adhered to its surface, rose in one single bubble; and the loss of weight indicated, without any correction, a specific gravity of 2·261, which differs very little from that I mentioned before.

† Annales de Chimie, Vol. LVIII. p. 75.

Château

Château des Ternes, and is now a boarding-school for young ladies. A gardener discovered it in digging up the ground, at less than two feet deep. This was all the information we could get; and nothing since has been discovered, that can lead to the slightest conjecture respecting the time or circumstances of its being buried there: but the singularities it exhibits sufficiently excite the curiosity of the antiquary, the naturalist, and even the artist, to induce us to attempt to satisfy it, by an examination of what remains of it.

Described

It is a head sculptured out of a piece of flint, of the same nature and appearance as that of which gun-flints are made. From the point of the chin to the crown of the head it is 9 cent. [$3\frac{1}{2}$ inches], from the forehead to the back of the head 76 mill. [3 inches], and its circumference, taken above the nose, is 236 mill. [$9\frac{1}{4}$ inches].

A hole 13 mill. [$\frac{1}{2}$ an inch] in diameter, in the lower part, and still partly filled with gypsum mixed with lime, appears to have served the purpose of uniting this head with the body of the figure, probably formed of another piece of flint, or perhaps of some substance more easily wrought; and which, according to the usual proportions, must have been 54 cent. [21 inches] high; so that the whole statue would have been 63 cent [$24\frac{1}{2}$ inches].

From the form in which the hair is dressed, it appears to be the head of a man. The hair is short, and confined by a simple, narrow band, such as the Greeks and Romans wore; which, added to the style of the figure, seems to indicate an antiquity considerably prior to the times of the Gauls; though the apple of the eye is marked out, which very rarely occurs in really ancient works.

Its age.

But we shall leave to more competent judges the discussion of these points, of which we thought a brief mention necessary, to render the description of the stone complete, and place in its proper point of view the question, that has principally engaged the attention of the class.

The flint covered with a thin coat.

The flint, of which this head is made, has been covered, in all the parts that have neither been broken nor worn away by friction, with a fine white coating of a scarcely perceptible thickness, attackable by no acid, and uniting with a hardness at least equal to that of calcedony the glassiness

of

of an enamel, sufficiently transparent to allow the different shades of the silex, more or less gray or bluish, to appear through it in some places.

Is this natural or artificial?

Is this covering, for I do not think it can be termed a *crust*, the work of nature, or of art?

Obstacles to its analysis.

It is obvious, that we cannot refer to analysis, to solve this question; for thus the fragment must be destroyed, and even then we should not obtain a sufficient quantity of the covering to afford unequivocal results. Nay, should they be certain and easy, they could inform us of nothing more, than we know already by its external characters of colour, opacity, hardness, and unalterability in acids, that its constituent parts are the same as those of calcedony.

Apparently an enamel.

The first idea that suggests itself on the inspection of this head is, that the block of flint, after having been laboriously cut on the wheel, in the same manner as gems, received a covering in the fire of the same nature as that applied on the biscuit in making porcelain. Not only do the glassiness of the enamel, and its thinness, appear to afford grounds for this opinion; but it is supported by comparing its shining surface with the dullness of the white crust, on two fractures occurring at the bottom of the left cheek, this crust having been formed evidently since it was buried in the earth.

But the flint has never been exposed to a strong heat.

But a large and more recent fracture on the right side exposes the silex retaining all its ordinary characters; and it is well known, that this substance loses its colour and transparency in a fire incapable of fusing even feldtspar. The fragment I subjected to this trial was exposed to a heat of 13° of the pyrometer only, when it separated into several pieces, and assumed the appearance of a biscuit to its interior parts.

Probably therefore natural.

This no doubt has led to a more general adoption of the opinion, that the calcedony covering the silex can have been deposited on it only in the humid way, during its having been in the ground.

Before I embraced it, I thought it necessary to search among collections of minerals of the same kind, for indications, at least, of the possibility of such a covering being a natural production.

In

But it does not resemble the natural crusts of other flints.

In these collections, flints commonly appear encrusted rather than covered; and the crust is dull, adheres to the tongue, imbibes acids, and even exhibits some signs of effervescence. There are some, it is true, covered with a very hard calcedony; but it is always thicker, less transparent, forming an uneven crust, exhibiting only a few shining parts in the fractures where traces of friction are perceptible, and never possessing the glassiness of enamel.

Calcedonies formed on smooth surfaces have not the polish of this;

It might be supposed, that the polish of the cut flint occasioned the glassiness of the calcedony that covers it, and produced the difference appearing in the earthy calcedony, that covers the two fractures: but the calcedony observed on several rock crystals, on mamillary agates, and moulded on cubes of fluate of lime or other crystals, the surfaces of which may be considered as polished, never have this shining aspect.

nor stalactitic,

The stalactitic calcedony of Geysser, in Iceland, is equally without the appearance of enamel, even on surfaces that have been in contact with flat bodies.

nor hydrophanous calcedonies;

The hidrophanous calcedonies, observed as forming transitions in pitchstones, chert, &c. are likewise of a dull white, frequently even in the recent fractures. They are always found in veins too, never as crusts. The same may be said of the opals, the fracture of which, though of a more vivid lustre, is always unequal, undulated, and exhibits no appearance to the eye approaching the lustre given by polishing.

nor opal.

Two calcedonies with polished surfaces were not crusts,

Two specimens however offered me a surface of sufficient polish, to give no hope of finding in nature a silex analogous to this antique. One came from the department of the Indre and Loire. It appeared entirely covered with white calcedony; but on breaking it, to examine its interior, I perceived only a continued mass of the same nature, the surface of which had acquired its polish solely from friction, which excluded all comparison.

and polished by friction.

The other specimen came from Siberia. One of its surfaces approached somewhat more in appearance a vitrified enamel, and had a tolerable lustre: but it was quite as foreign to flint and its transitions, since it was nothing but an opake white calcedony, on a calcedony more transparent.

It

It was likewise intersected by reddish lines, crossing each other in different directions, as in the ludus Helmontii.

The doubts arising from this comparative examination induced me to inquire, whether it might not be the work of art, at least how near art could approach it. Attempts to imitate it by art.

I have already noticed the facility with which silex is altered by fire; we could not think therefore of employing the processes by which porcelain is covered. But might not the same end be obtained, by cementations with a moderate heat, long digestion, saline fusion, or combined solutions, to set at work powerful affinities? Chemical experiments alone could throw light on this subject, and to them I had recourse.

It will be sufficient to give a brief account of the results of the first unsuccessful attempts.

Flint cemented in lime from marble, sulphate of lime, sulphate of alumine, and muriate of soda, underwent no alteration, as long as the heat was not carried to a certain point. Beyond that it began to lose its colour, transparency, and tenacity. Unsuccessful trials.

A fragment of flint treated with caustic potash in a platina crucible experienced only a trifling diminution of weight, more or less in proportion to the time in which it was continued in a heat sufficient to keep the potash in fusion.

Alumine being one of the constituent parts of calcedony, though in a small proportion, I conceived, that by treating the flint with a solution of potash saturated with alumine, and adding a portion of free potash to act on the silex, the affinity these two earths have been shown to possess for each other*, and with a common solvent, must effect on the surface of the silex a new combination, at a temperature incapable of altering its nature. Alumine with excess of potash,

Considering on the other hand, that analysis had developed in some calcedonies the presence of lime, I put a small quantity with another piece of flint into a similar preparation of potash and alumine. and the same with the addition of lime, gave flint a similar coat,

These two experiments were made in platina crucibles, and the success exceeded my expectations, though they were

* See our Journal, Vol. IV. p. 16.

not

not preceded by any trial, to adjust the doses of the agents, and the duration and intensity of the fire. The flint was not altered internally; it only acquired a very thin coat on its surface, of a uniform thickness, united into one body with the mass, unattackable by acids, and of such hardness, that it rapidly wore away the stones used by lapidaries, and was as impenetrable to adamantine spar, or corundum, as the coating of the sculptured head.

of great hardness,

The pieces came out of the crucible of a dull white, as I expected; but some parts, which I had polished in the same manner as hard stones, showed them to be capable of as good a polish as the head found at Ternes.

and capable of a fine polish.

It cannot be denied, that such a complete imitation favours the opinion of the coating having been a work of art. It is not necessary for this to suppose, that the chemical affinities, which led to this imitation, were known to the artist who executed the antique: for it would not be the first process found out by loose trials, and practised with success for centuries before the true theory was discovered.

The artist might have employed this process, though ignorant of its theory.

This opinion however has not obtained general assent. Those who contest it rely chiefly on the resemblance of the coating of several flints found in the environs of Ternes, specimens of which were shown to the class by Mr. Chaptal; and which in fact exhibited on some of their faces parts of an enamel, if not equally uniform in tint and in thickness, at least as glossy.

This opinion still questioned

Flints found with a nearly similar coat.

Others have thought with Mr. Fourcroy, that, whether the coating of the sculptured head were formed in the earth, in the same manner as the crusts of these flints, or added by an artificial process after it came out of the sculptor's hand, it must be admitted to have received its polish from art; and that this was the only way of reconciling the inferences we are obliged to deduce from its present state.

Still apparently polished at least by art.

Under these circumstances, the committee can only propose to the class to suspend its judgment, and to leave the subject open to farther inquiry and discussion, for the solution of a question interesting to the history of the arts, and to the sciences of the antiquary and the naturalist.

XI.

XI.

Experiments on Double Vision by Dr. Haldat, *Secretary to the Academy of Nancy.**

Vision with a single eye vulgarly supposed to be most distinct.

THE superiority of single vision, or vision with one eye, over double vision, or with both eyes, has long been the subject of two opposite opinions. It is maintained by the vulgar, that single vision is most distinct. Philosophers on the contrary assert, that we see better with two eyes, than with one. The latter opinion, established by father Cherubin in his treatise on distinct vision, and placed beyond all doubt by Dr. Jurin, by means of an experiment, which consists in looking at a sheet of white paper, with a piece of pasteboard, or other opake substance, affixed to the right temple, and projecting so far forward, as to conceal half the sheet of paper from the right eye, while the whole is visible to the left. On looking at the paper alternately with one eye and with both, we perceive very distinctly, that the part seen by both eyes is much brighter, than that seen by the left only: the former appears with all its natural whiteness, while the latter appears as if shaded by a thin gauze. Dr. Jurin even estimated the intensity of this obscuration by a very ingenious photometric contrivance.

The contrary proved by Jurin.

Binocular telescope.

If this experimental proof of the superiority of double vision over single wanted farther support, we might adduce the experiments made by means of binocular telescopes, the superiority of which have been acknowledged by all observers, both for distinctness of vision and magnifying power, over single instruments magnifying equally, and of equal clearness.

A double sensation produces a single perception.

From this well established fact, that double vision produces a more vivid and distinct sensation than single, it follows, that the sensation produced by the impulse of light on one of the eyes is reinforced, if I may use the expression, by that produced on the other; and that consequently a complex sensation may give rise to a simple perception. But does this faculty of forming simple perceptions, when the impressions are complex, equally take place in all cases? Is it the case when the impressions are heterogeneous, as well

But may two different sensations be blended into one?

* Journal de Physique, Vol. LXIII. p. 387, Nov. 1806.

well as when they are homogeneous? Such is the question I propose to resolve. I confess, however, that I was led to the experiments, that constitute the subject of this memoir, by an accidental circumstance, on occasion of an eclipse of the sun, the progress of which I set myself to observe. The instrument I used not being furnished with a coloured glass, my right eye was so much affected by it, that it was deprived of distinct vision for some days. When it began to recover, all white objects appeared to me to have changed colour, and acquired a reddish hue, the depth of which I could alter at pleasure, by looking at them with both eyes, or with the affected eye alone. When I looked at them with the right eye, which had been overstrained, they had a red hue; when with both eyes a rose colour; when with the left eye only, white as usual. This fact, the explanation of which is foreign to my subject, led me to conclude, not only that the perception produced by the impulse of homogeneous light on one of the eyes was reinforced by that from a similar impulse on the other; but that the impression of heterogeneous rays on each of the two eyes might give birth to a complex perception, which, being composed of both sensations, would be a mean between the two.

An eye viewing an eclipse without a dark glass, its sight was destroyed for a time, and afterwards represented objects red.

This blended with the natural hue, when both eyes were used.

Desirous of satisfying myself whether the impression of all the primitive colours, applied separately and simultaneously to both eyes, would constantly produce a complex sensation analogous to that I have just related, I determined to procure myself transparent coloured mediums, which, suffering rays of one sort only to pass, might, by being applied separately to each eye, subject this double organ to a complex impression. The difficulty of procuring myself coloured glasses of all the tints, or colours sufficiently transparent to paint similar ones to those of magic lanterns, induced me to reject these, which would have been more convenient, and have recourse to hollow quadrilateral prisms of white glass, into the cavity of which I poured liquids of a proper colour and tint for all my experiments.

Experiments on different colours interposed between the eye and the object.

The choice of tingeing substances for colouring the water, with which the prisms were filled, requires some precautions, of which it may be proper to inform the reader; not

Choice of colouring substances.

not only because all colouring substances have neither the same solubility nor the same transparency, but because they have not all the same tint by reflected and refracted light; as Spallanzani observed with respect to the globules of the blood, which appear red or yellow in the microscope, according to the manner in which they are acted upon by the light. It has long been known, as may be seen in Newton's Optics, that the lignum nephriticum exhibits a phenomenon of this kind. The infusions of violets and litums, which have a pure blue tint by reflected light, have a decided violet by refraction. But it is particularly difficult to obtain the desired tints among the yellows. Those that are the purest yellow by reflected light have a decided orange by refracted. They can only be divested of this red hue, that alters them, by filtering them a great many times, after diluting them with a considerable quantity of water. This effect, which depends apparently on the opacity of the colouring particles, and the force with which they repel the most easily reflected rays, and admit only those that are the least, seems to me well adapted to explain most facts of this kind. In reality it is the least refrangible colour, the red, that generally produces those differences observed in coloured mediums by reflected and refracted light; and these differences are diminished by weakening the tinctures, and diluting them with water.

Many not the same by reflection and refraction.

Blood.

Nephritic wood, violets, litums.

Yellows.

The red rays generally the cause.

Two prisms of glass being filled with different coloured liquors, and applied one to each eye, if we direct both eyes at once to the same object, we receive a double impression, the perception corresponding to which is simple, and that of the colour resulting from a mechanical mixture of analogous colouring substances. Thus a yellow prism, and a red prism, applied one to the right eye, the other to the left, produce the sensation of orange, as a mixture of vermilion and yellow ochre would do. But not to enter into tedious details, I shall give a tabular view of the results of the numerous experiments I made on this subject.

Glass prisms, filled with coloured liquors applied before the eyes.

As the results of these experiments cannot be exact, unless the colours be distinct and pure, in reflecting the light toward the eye we ought to exclude two kinds of bodies, those that are too bright, and disturb the sight by their

Cautions.

glare; and those which, reflecting colours proper to themselves, vitiate the results: such as candles, lamps, &c., which diffuse too abundant light in yellow rays. The best method is to take a piece of white paper, about eight inches in diameter; place it on a black or brown ground at the end of a moderately light room opposite the window, and, standing two or three yards from it, with the back to the window, look at it with both eyes, each having its proper prism before it. In this manner I obtained the following results.

Method of making the experiment.

Results of the combined perception of different colours by refraction.

Red and yellow	produced orange.
Red and orange	aurora.
Red and blue	violet.
Red and violet	a pleasing rose colour.
Red and green	a muddy red.
Red and indigo	an indeterminate colour.
Orange and yellow	light orange.
Orange and blue	muddy green.
Orange and green	light green.
Orange and violet	muddy rose colour.
Yellow and blue	muddy green.
Yellow and green	light green.
Yellow and violet	harsh red.
Blue and green	sea green.
Blue and violet	a deep violet.

The colouring matters that were employed.

In order to enable those who wish to repeat these experiments to execute them with greater facility, and render the results uniform, I shall add here an account of the colouring substances, that were employed. The red was a decoction of brazil brightened by an acid. I likewise employed for this colour red wine, and a decoction of cochineal. The yellow was prepared from quercitron bark, the decoction of which must be weak, well filtered, and brightened: the orange, from French berries, or turmeric: the blue was *aqua cœlestis*, a solution of copper in ammonia: the green, an infusion of mallow flowers changed by potash: the violet was prepared from litmus and violets. These experiments are easy to execute; they only require a little practice, and the habit of distinguishing different tints of colour.

After

After having thus proved, that coloured rays of a different nature, obtained by means of refracting mediums, produce the perception of a mixed colour by their separate action on the eyes; I was desirous of satisfying myself, whether these effects would equally take place from light reflected by different bodies, and received immediately by the eyes. But as it was necessary for this purpose, that the organs should be placed in such a situation, as to render the impression received by each eye totally unconnected with that received by the other, I separated the bodies subjected to the experiment by a thin opake plane, placed perpendicularly between the two eyes. The little apparatus I employed consisted of a square piece of wood, on the middle of which was placed a very thin vertical plane, the upper edge of which was applied against the forehead and nose, so as to separate the two eyes. The whole of this apparatus, which was twelve or fifteen inches high, was painted black in distemper. The coloured surfaces, the double impression of which was to be observed, were placed parallel to each other on the base, one on each side of the vertical plane. These coloured surfaces were little pieces of pasteboard ten or twelve lines square, painted in distemper, and representing the primary colours. It is necessary to have some smaller, and some narrower, and particularly to be provided with at least three shades of each colour.

Experiments with reflected light.

Apparatus.

The apparatus for double vision being placed opposite a window, and the pasteboards on each side of the base, the forehead is to rest lightly on the upper edge of the vertical plane, and then, *viewing both objects with great attention at the same instant*, the effect of the double impression will be perceived. The phenomena that accompany or precede the complex sensation resulting from it are worthy notice.

Method of using it.

Phenomena.

1. When with steady attention, for about half a minute, or even longer if necessary, we see the objects evidently approach each other, and the plane that separates them disappearing, they gradually encroach upon each other, till they are entirely confounded together, if the distance from which they are observed be in proportion to the magnitude of the little pieces of pasteboard; as that of twelve

The objects appear to approach each other,

or

till they begin to overlap, and then blend at once.

or fifteen inches. 2. In this apparent progress of the bodies toward each other, they approach with a pretty regular motion, till they partly cover each other; when we see them suddenly confounded together, as at one leap; and then the compound sensation is changed into a simple perception, and only one single object is discerned, the colour of which is the result of the combination of the colours of the two pieces of pasteboard. All the primary colours, subjected to the same trial, afforded me analogous results, indicated in the following table, a few modifications excepted, which I shall notice.

Results of the combined perception of different colours by reflection.

Red and yellow	produced orange.
Red and orange	a bright aurora.
Red and blue	violet.
Red and green	a rosy green.
Red and violet	a rosy violet.
Red and indigo	a dingy violet.
Orange and yellow	light yellow.
Orange and blue	muddy green.
Orange and green	reddish green.
Orange and violet	light violet.
Orange and indigo	harsh violet.
Yellow and blue	faint, indeterminate green.
Yellow and green	light green.
Yellow and violet	muddy green.
Yellow and indigo	a dingy green.
Blue and green	deep or light green, according to the shade of blue.
Blue and violet	deep violet.
Blue and indigo	deep blue.
Green and violet	a dingy violet.
Green and indigo	a very deep blue.

The experiments require practice and attention, and are fatiguing to the eyes.

These experiments, though not difficult to execute, require a certain practice, and steady attention, without which they will not succeed. The strong convergence necessary to be given to the optic axes renders them fatiguing. I have met with several persons, who, not being able to keep up their attention, and view the two objects steadily at one time, did not experience the compound sensation,

or

or the perception resulting from it: practice however has rendered it very familiar to me, as well as to several persons, whom I have employed to repeat them.

All colours do not blend with equal ease.

As the perception of the mixed colour in these experiments results from the impression made by two objects of different colours, and received simultaneously by each organ of vision, it would seem, that all colours, being equally capable of producing such an impression, should occasion a sensation equally complete and distinct, and produce it with equal facility. This however is not the case: several of them combine but imperfectly, or not at all. The combination of blue and yellow for instance is not only painful, on account of the continued attention it requires, but the colour resulting from them is vague, nearly indeterminate, and of a disagreeable hue. This singular anomaly, the most remarkable that occurred in these experiments, is not sufficiently accounted for by the extreme difference and heterogeneousness of blue and yellow; since blue and red, which are equally heterogeneous, combine easily and completely to produce a violet. The property of illuminating, which these colours possess in different degrees, confirmed by Newton, and subsequently by Herschel, is the only circumstance, that appears to me capable of giving a plausible explanation of it: for this property of illuminating depends on the force with which the colours act on the eye. Thus when two colours possessing this property in different degrees act at once on the two eyes, the too powerful impression on one necessarily renders that on the other less sensible, and the mixed colour produced by this double impression will not therefore be very distinct. This appears to me the better founded, as the green is more distinct when the yellow is weaker, and the greenish tint produced by the combination of blue and yellow appears to contain much more of the latter colour than of the former: and further, if the impression of the yellow colour be weakened by the interposition of a semitransparent substance, the green is rendered much more determinate. Both yellow and orange combine very difficultly with blue and violet, while they combine together, or with red, very easily.

Blue and yellow, painful and imperfect.

This owing to a difference in the propertyof illuminating.

Hence blue unites best with a weak yellow,

the yellow has more than its due effect,

and the best green is produced by obscuring the yellow.

Homogeneous

Homogeneous colours combine best.

Homogeneous colours of different shades combine with the greatest facility. Thus a strong red and a faint red give a mean tint of red. What appears very astonishing is, that black and white comport themselves as pigments of the same colour would do. White, which is the result of a union of all the primary colouring rays, renders colours lighter, as a mechanical mixture of a white powder would the pigments that represent the other colours. Thus red and white give a flesh colour. Black, which is merely the absence or privation of colour, might be supposed to produce no effect on the organ of sight; yet it has just the same as a mixture of any black powder would with the pigments that produce other colours. Thus light blue or green with black gives the perception of dark blue or green. Glaring colours, such as red and orange, less readily associate with black: but white and black, the white pasteboard being placed on a black ground, and the black pasteboard on a white ground, produce the sensation of gray, like a mixture of ivory black and chalk.

Black and white unite with and modify all colours.

I should tire the reader, if I were to relate all the differences I have observed in colours with respect to the degree of facility with which they combine, or rather associate the effects they produce separately on the two eyes. But I cannot pass over another class of facts, which pertain to the same theory, and may serve to elucidate it. These facts relate to the double vision of objects resembling each other in colour, but differing in form, or differing both in form and colour at the same time. Little parallelograms of pasteboard, either black, white, or of various homogeneal colours, twelve lines long and four broad, placed on the opposite sides of the vertical plane of the apparatus, one parallel the other perpendicular to the plane, exhibit the appearance of a cross with equal arms. Two equal disks, eight lines in diameter, placed on opposite sides of the same vertical plane, are so blended together, that it is impossible to distinguish them. Parallelograms like those just described, or squares of unequal size, if of different colours, exhibit by double vision crosses or concentric squares, the place of junction or superposition of which exhibits the mixed colour.

Double vision of objects differing in figure.

Parallelograms placed in opposite directions

form a cross,

and their colours blend where they join.

I shall

I shall not enlarge on this article, as theory alone is sufficient to determine all the cases of alteration of figure by double vision; for it is sufficient to imagine the two objects placed one upon the other, the effect which double vision produces.

In all cases one object seems placed on the other.

I was not satisfied with thus combining the primary colours by pairs only, but extended my researches to more numerous and complicated associations, which led me to some remarkable results, that could not have been deduced by analogy alone from the simple association. The first of these results is, that it is possible to receive at one time two distinct and comparable sensations by the simultaneous impression of several objects on our eyes. Little parallelograms of red, blue, yellow, and green, placed parallel to each other, and on opposite sides of the vertical plane of the apparatus, in the order I have mentioned, and subjected to double vision, give the perception of orange, produced by the association of red and yellow, and of sea green, produced by the green and the light blue.

More than two colours.

Four colours give two objects.

The second remarkable result is, that the colours, in their apparent associations or combinations, seem obedient to a kind of affinity, by virtue of which those that have most analogy to each other combine in preference, be the disposition of the parallelograms what they may. Thus the yellow combines with the red, and the blue with the green, let them be disposed in either of the following modes: yellow, blue, red, green; or yellow, blue, green, red; or blue, yellow, red, green; arrangements in which the colours are obliged to jump over one another, if I may use the expression, to seek out and combine with those analogous to themselves. This tendency of certain colours to combine together in preference to others appears to me to be the principal cause, that opposes the recomposition of light by placing all the primary colours on each side of the vertical plane, which I at first hoped to have effected.

They have a sort of affinity, by which they seek out those most congenial to each other.

This perhaps prevents the combination of all into white light.

The experiments on double vision by means of reflected and refracted light suppose a third class, composed of a combination of the processes of the other two; but, as there is no essential difference in the results, I proceed to explain these phenomena.

The effects of reflected and refracted rays together are similar.

The

Enumeration of the facts.

The facts established by our experiments, and of which we have to give the proof, may be reduced to the following.

1. Simultaneous and separate double vision of objects differing in colour produces a mixed or compound sensation, which gives rise to a simple perception, similar to that which would be produced by a mechanical mixture of colouring substances, representing the colours combined.

2. Objects differing in figure, and similar in colour, combine their figures, as objects of different colours do their colours.

3. Colours in their association, or apparent combination, by double vision, seem to obey a kind of affinity, which renders it more easy between some than between others.

Reason why a simple perception is produced by a compound sensation.

The explanation of the first fact is deducible from the common laws of our sensations. Perception being generally proportional to the sensation, and this to the impression made on our organs, a stronger impression must occasion a more lively sensation, and consequently a proportional perception. Hence when an impression is double, from being received by both eyes, it must be heightened. The perception however will not be double, because we distinguish similar impressions with difficulty; and under the same circumstances, and by the same agents, we are susceptible of impression only to a certain degree.

The facts respecting the apparent combination or association of objects of heterogeneous colours, by artificial double vision, offer several questions to be solved with respect to their cause. The first and chief, with which all the others are connected, is to know why, in these experiments, a double heterogeneous impression does not occasion a double perception, as when we see two objects with both eyes at once; and why on the contrary there is but one perception, as when we see a single object with both eyes.

Why do we not see two objects in this case, as when both are seen by each eye?

Hypotheses of physiologists on single vision.

To explain this common phenomenon of vision, physiologists have invented divers hypotheses, which I have attempted in vain to apply to the explanation of my experiments. Some have asserted, that perception was simple in consequence of a union of the optic nerves, which, being dependant on each other in their functions, could therefore

Union of the optic nerves;

produce

produce only a single perception: others, that, as similar impressions cannot be distinguished, we have but one perception, though there are two impressions. But it is easy to see how far these hypotheses are from affording a satisfactory explanation of the phenomena: for it is evident, that, according to the one, we can in no case distinguish similar objects; and according to the other it would be impossible for us to be sensible of the compound perceptions established by our experiments.

and in capacity of distinguishing similar perceptions.

These unsatisfactory.

The explanation of the physiologists who tell us, that perception, being received in a simple subject, cannot be otherwise than simple, by no means elucidates the question. Whatever opinion may be entertained of the cause of our sensations, it is certain, that we have within us a power that tends to individualize, to identify with our being, the different sensations we experience at the same time. Thus in a piece of music we do not distinguish the sound of any one instrument in particular, or the effect of any of the parts that compose it: we receive only a simple perception, resulting from these manifold and simultaneous impressions. Thus two dishes, in which the refinement of luxury has combined substances of the most heterogeneous nature, occasion only a mixed taste, without our being able to distinguish any of those of which it is compounded. But desirous of obtaining a more accurate knowledge of this faculty of identifying and combining simultaneous heterogeneal sensations, I attempted to make experiments on the smell and taste analogous to those on the sight, and this by means of heterogenous flavours and odours of equal solubility and volatility.

as well as that perception is simple, because received in a simple subject.

Faculty of combining into one many simultaneous sensations,

as of sound,

and of taste.

Experiments of this kind on the taste and smell scarcely practicable.

As I was unable to preserve the action of heterogenous agents on these senses sufficiently distinct, to obtain satisfactory results, I attempted analogous experiments on the sense of hearing, which, as it consists of a double organ like that of sight, was better adapted to my experiments. I took two leaden speaking trumpets, covered exteriorly with wet cloths, and introduced their extremities, wrapped round with tow, one into each ear. These were employed to isolate the sounds of two monocords, which I introduced

Experiments on the hearing.

Apparatus.

duced into them, so that the sounds excited in one of the organs were kept distinct from those excited in the other. On making the strings of these little instruments vibrate at the same time, I convinced myself, that the different impressions produced by different sounds combined in the same manner, as when they are received simultaneously by the same ear. The monocords tuned to thirds, fourths, or fifths, to each other, produced the perceptions corresponding with those concords. I know not whether it were prejudice, but these concords seemed to me better united, and more harmonious, that when received by the same ear.

Sounds impressed on each ear separately combine.

These facts, and many others of a similar nature, leave no doubt, that we enjoy the faculty of identifying or combining heterogeneous sensations. However, as we can likewise experience a distinct perception of heterogeneous and simultaneous impressions, it is evident, that this faculty is not so inherent in our organization, as to be independent of certain circumstances, without which we cannot produce the effects that characterise it. Thus we cease to experience the perception that results from the apparent combination of heterogeneous colours, when the objects are at too great a distance from the vertical plane that separates them, or of too great extent.

This faculty dependant on circumstances.

Instance in colours.

These circumstances well considered have enabled me to reduce these facts to the common laws of vision, according to the theory of Buffon and Condillac, which is that of the majority of physiologists. Objects according to these philosophers appear to us single, though there is a double representation of them, one at the bottom of each eye, because the touch, which corrects the judgment formed by the sight, teaches us, that the object, which we see double, is notwithstanding single. Habit and experience have rendered this manner of seeing so necessary, that it is impossible for us to change, without disturbing the order established between the sight and touch. But as every object, that produces a double impression on the retina, is necessarily in the point of meeting of the optic axes, and consequently painted on corresponding points of the retina; and that it is with respect to this correspondence of the

We see objects single at first because our judgment is corrected by the touch, afterward from habit.

the images on the two retinas, that the habit of judging an object seen double to be single is established; an object will appear double, whenever this correspondence does not take place. This happens when we press upon one of the eyes in such a manner, as to determine the impression of the image of a single object to parts of the two retinas that are not commonly acted upon simultaneously. This happened, as Cheselden relates, to a man who had one of his eyes distorted by a blow; and he saw objects double, till habit and experience had done for the new points of correspondence on the two retinas, what they had before effected for the points that corresponded previous to the accident.

Hence a single object appears double when the correspondence of its impressions on the eyes is altered,

till the new correspondence is learned from habit.

Double objects will appear single on the contrary, when, their impression being made separately and simultaneously on corresponding points of the retinas, we experience the same impressions, as would be excited in us by the picture of a single object. This is what takes place in our experiments: for the two eyes, in consequence of the vertical plane that separates them, receiving separately different and simultaneous impressions on points of the retinas which are the same, or nearly the same, as those that correspond by habit to the two images of a single object, must excite in us the idea of the presence of a single object only. What convinces me that this association or combination of the double image depends on its being depicted on corresponding points of the two retinas is, that, when this correspondence is destroyed by placing the objects at too great a distance from each other, or by giving them too great extent, the association does not take place, and we have a distinct perception of the images of both objects. This in fact must be the case, for the rays proceeding from one object to both eyes necessarily observe certain proportions of correspondence, which cannot exceed a given limit.

Hence double objects appear single by means of this correspondence,

as in the preceding experiments.

The superposition and apparent motion of the two objects one toward the other is an illusion produced by the force of habit, which, having constantly taught us, that objects, the double image of which is painted on the bottom of our eyes, without exciting a double perception, are placed in the point of meeting of the visual rays, transfers them to

The motion of the objects toward each other an illusion.

the point where we see them habitually. This allusion is the more unavoidable, because a single perception always corresponds to a double image, and a double image is necessarily produced by a single object seen by both eyes; but in our experiments, from the interposition of the vertical plane, one impression only is received from each body, while without this plane we should receive two impressions from each, that is to say a quadruple image would be formed.

The combination of colours accounted for.

The apparent combination of heterogeneous colours is another necessary consequence of this allusion, of this apparent displacement of the objects; for we experience the sensation of mixed colours, as often as their united elements produce their impressions conjointly on the organ of sight. Thus a mixture of blue and red wool gives a violet colour to the cloth made of it. Here, though the colours act separately on each of the two eyes, it is on points, the correspondence of which is so confirmed by habit, that only one perception can result from them, which is consequently composed of the different effects of the double impression.

That of figures from the same cause.

I say nothing of the phenomena of the association or combination of figures, its theory flowing so naturally from what has already been laid down, that a particular explanation of it would be superfluous. As to the greater or less facility, with which heterogeneous colours combine by double artificial vision, it is explicable in the same manner as the different effects of concords on the ear. In the same manner as there are sounds, the association of which is disagreeable, because their proportions are perceived with difficulty, there are colours, the heterogeneousness of which renders their association laborious, and consequently not pleasing. I cannot discuss this question more amply, without entering into the depths of the theory of sensations, and wandering from my object, which was to make known, and to reduce to the common laws, some remarkable facts, that add to the number of illusions we experience from the wonderful but not very accurate organ of sight.

Affinity of colours simliar to that of concords in music.

Experiments

XII.

Experiments on the Effects produced by a high Temperature on the Animal Economy: by F. F. Delaroche, *of Geneva.*

(*Concluded from p.* 149.)

Sect. II. *Of the Degree of Heat Man is capable of supporting.*

Physiologists have paid much more attention to the effects of heat on man, than to those it produces on other animals. The extremely interesting experiments of Drs. Fordyce and Blagden leave little to be desired on this head: yet I thought it not amiss to repeat some of them, in order to determine whether other individuals would be able to endure heat as well as those gentlemen. For this purpose Mr. Berger and I shut ourselves up in a room heated by a stove, our bodies being naked, and defended from the radiant heat by a linen screen. We estimated the temperature by a thermometer hung against the wall, four feet nine inches from the floor.

Experiments of Dr. Fordyce, &c. repeated.

Experiment I.

Temperature of the room at the beginning -	189° ·5 Fahr.	Temp. from 189° ·5 to 194°
at the end - -	194	
I entered the room at - - - - - - - - -	3 h. 17 m.	8 minutes,
I left it at - - - - - - - - - - - - -	3 h. 25 m.	
Weight of my body when I entered -	128lb. 11oz. 3gr.	loss in weight 6oz 3gr.
ten minutes after I came out -	128lb. 5oz.	

On entering I felt pretty sensibly the impression of the hot air, but without being inconvenienced. In four minutes a few drops of sweat appeared on my forehead. In five minutes all my body was covered with a copious perspiration. At this time I began to feel a little weakness, and difficulty of breathing, which continued to increase, and obliged me to quit the room.

Other effects.

Experiment II.

Temperature of the room at the beginning -	194°.	From 194° to 189° ·5,
at the end - -	189° ·5.	

Mr.

13 minutes, Mr. Berger entered it at - - - - - - - 3h. 41m.
left it at - - - - - - - 3h. 54m.

loss near 11 oz Weight of his body when he entered - 105lb. 2oz.
five minutes after he came out - 104lb. 7oz. 6gr.

Other effects. On entering he felt a slight burning heat at the nostrils and round the nipples. The perspiration, which began to appear on his forehead in four minutes, flowed copiously from all parts of his body two minutes after. At coming out he felt a little weakness and even faintness. His pulse the instant before beat 128 in a minute.

Experiment III.

From 228° ·8 to 225° ·5, Temperature of the room at the beginning - 228° ·8.
at the end - - 225° ·5.

7 minutes, Mr. Berger entered it at - - - - - - - 4h. 32m.
left it at - - - - - - - 4h. 39m.

loss 7oz. 1½gr. Weight when he entered - - - - - 103lb. 15oz. 3gr.
ten minutes after he came out - 103lb. 8oz. 1½gr.

Other effects. He felt a pretty sharp burning round the nipples, at the nostrils, and even all over the face. In four minutes he was covered with a copious and general perspiration. When he came out he was weak and ill. A moment before he was not able to count the beats of his pulse. Three quarters of an hour after he came out he had recovered his natural state.

Capacity of enduring heat differs greatly in individuals. These experiments agree entirely with those reported by Sir Charles Blagden in showing, that man is capable of enduring exposure to very high degrees of heat for a short space of time: but they show likewise, that there is a great difference between individuals with respect to this capacity. In these experiments Mr. Berger supported heat much better than I, as appears obvious on comparing them. On the other hand, if we compare them with those of the English philosophers, we shall find, that they, or at least Sir Charles Blagden, suffered much less from the heat than Mr. Berger himself. In fact in one experiment he endured for eight minutes a temperature between 240° and 260°, without experiencing more inconvenience, than Mr. Berger did after staying seven minutes in a temperature of 227° ·8. Another time Sir Charles Blagden was exposed for twelve minutes to a heat of 222°, without suffering any inconvenience but a little weariness.

Mr.

Mr. Delaroche made a great number of experiments too on the heat both men and animals are capable sf enduring in baths of hot water, and in vapour baths; on the influence heat exerts on respiration; on the connexion that exists between the evaporation of the perspirable matter and the faculty animals possess of producing cold; on the influence of heat on respiration; and on the state of the bodies of animals destroyed by excess of heat. Heat applied in various ways.

He concludes his labours with the following observations.

Such are the inquiries I have made, with a view to investigate the effects of a strong heat on men and animals. I could have wished to have extended them farther, and rendered them more complete: but the time such a labour would have required, and the difficulty attending it, did not permit me. I cannot therefore deduce from them general consequences, but I shall briefly recapitulate the results they afforded me. The subject not exhausted. General results.

1. The object of my first experiments was to determine the degree of heat requisite to destroy animals; and from them I learned, that small animals perished on exposure to a heat of 144° ·5, or even of 133° ·25. It is even probable, that a lower heat, but longer continued, would produce this effect. Small animals destroyed at 144° ·5, or even at 133° ·25.

2. Mr. Berger and I confirmed by experiments made on ourselves, the faculty that man possesses of enduring exposure to high temperatures, though but for a short time it is true. A comparison of these experiments with each other, and with those of Sir Charles Blagden, taught us, that the extent of this faculty might be very different in different individuals. Extent of the capacity of sustaining heat differs.

I. The experiments in which we exposed ourselves to the action of aqueous vapour enabled us to verify an observation of Dr. Fordyce, that the sensation of air loaded with vapour is much more painful than that of dry air at an equal temperature. Dry heat borne most easily.

4. We endeavoured to calculate with precision by weighing the effects of heat on perspiration. The loss of weight we experienced in this way appeared to be in the direct ratio of the increase of temperature. We found too, that the heat of aqueous vapour excited perspiration much more powerfully than dry heat. Perspiration in the ratio of the temperature.

5. I

The faculty less extensive than supposed,

5. I think I have shewn, that the faculty possessed by man and animals of preserving a constant temperature, though exposed to great heat, is much less extensive than has generally been supposed from the experiments made by Drs. Fordyce and Blagden; and that it is by no means comparable to the faculty they have of resisting cold, and preserving a temperature superior to that of the surrounding medium.

or than that of resisting cold.

What is its cause?

6. Though this faculty is limited, it is nevertheless real: it was an interesting inquiry therefore, to determine its cause. Does it reside wholly in the perspiration produced by evaporation, as some physiologists suppose? The experiments I have made render this opinion extremely probable, at least with respect to cold-blooded animals: but they have not enabled me to decide, whether it be the same in animals with warm blood. I have only found, that inanimate substances, the surfaces of which were entirely wet and susceptible of evaporation, acquired a less elevated temperature, when exposed to a high heat, than warm-blooded animals under similar circumstances.

Less oxigen consumed in respiration at a high temperature.

7. I afterward endeavoured to ascertain the influence of heat on the phenomena of respiration. Dr. Crawford, who investigated this subject very minutely, imagined he observed, that the vitiation of the air by breathing was proportionally less, as the heat to which the animal was exposed was greater. In a considerable number of experiments I made, I was not able to discover any constant proportion between the vitiation of the air in which animals were included and the temperature to which they were exposed.

Bodies dissected.

8. Lastly I turned my attention to the circumstances, that accompany death occasioned by exposure to heat; and I particularly examined the state of the bodies of animals thus killed. The phenomena that appeared on dissection however, among which the most remarkable was a great diminution of muscular irritability, were not sufficiently constant, to allow me to draw any conclusion respecting the cause of this death.

Muscular irritability much diminished.

Observations

XIII.

Observations on the Two different Methods of preparing Acetic Ether; by Mr. HENRY, *Professor in the School of Pharmacy at Paris*.*

BEING directed by the Society of Pharmacy to ascertain the difference between acetic ether prepared in the direct way, and that prepared by the intervention of sulphuric acid, I shall proceed to give an account of the experiments I made on the subject.

Gehlen says acetic ether cannot be made without a mineral acid.

Mr. Gehlen, in a letter addressed to Mr. Guyton, in the 79th Number of the Annales de Chimie, affirms, that he has proved the truth of Scheele's assertion, who says, acetic acid is incapable of forming ether, without the intervention of a mineral acid.

But very little suffices.

The author does not say what acetic acid he employed; he merely asserts the fact, recommends "the use of very pure acetic acid," and adds, "that a minimum of sulphurous acid is sufficient to form ether."

The German acetic acid chiefly from acetate of soda.

I know that in a great part of Germany acetic acid is extracted from acetate of soda by means of sulphuric acid; while that directed by Pelletier is obtained from acetate of copper. I employed the latter, and it did not contain an atom of mineral acid. But without entering into the discussion of a point long decided by uniform facts, I return to the object of my inquiry, the examination of the two kinds of acetic ether.

Pelletier's process for acetic ether.

We are indebted to Pelletier for the process for obtaining acetic ether, which consists in mixing equal parts of rectified alcohol and acetic acid; in cohobating the product of the distillation on the residuum three times; and in rectifying the ether from the potash.

I followed his process with this difference, that I carried the cohobation as far as six times.

Its results.

From a mixture of 500 grammes [a little more than a pint] of alcohol rectified to 36°, and an equal quantity of acetic ether at 11°, I obtained 495 grammes of ether at

* An. de Chim. Vol. LVIII. p. 199, May, 1806.

24°, immiscible with water, of a pleasant smell, and powerfully reddening vegetable blues. No particular gas was evolved during the operation; the atmospheric air alone being displaced by the gasiform ether. I rectified this ether over potash purified by alcohol; after which it no longer reddened blue vegetable colours, indicated 25° by the areometer, and weighed 420 grammes.

Durosier's with sulphuric acid.

The process by the intervention of sulphuric acid, pointed out by our colleague Durosier, consists in introducing 500 grammes of powdered acetate of copper into a tubulated retort, and adapting to it a Woulfe's apparatus. 500 Grammes each of rectified alcohol and sulphuric acid are then mixed together, and when cold are poured through the tubulure into the retort; heat is gradually applied; and 640 grammes of acetic ether are immediately obtained, mixed with a small quantity of sulphurous acid. This ether marks on the areometer 25½°, powerfully reddens vegetable blues, and forms a precipitate with barytes or lime water. During the process a small quantity of elastic fluid is disengaged, which I found to be sulphurous acid gas.

Examination whether it contained sulphuric ether.

I rectified this ether with 50 grammes of potash purified by alcohol; and, to ascertain whether any sulphuric ether existed in it, I separated what came over into portions of 50 grammes each.

Gravity of the product differed at different periods.

The first portion indicated on the areometer 31°, the second 28°, the third 27½°, the fourth 26½°. These different products together indicated 28°, and weighed 535 grammes.

Gravity of sulphuric and acetic ether mixed near the mean.

To find whether it were easy to detect the presence of sulphuric ether in acetic ether by separating the products, I made a mixture of 50 grammes of the former at 56°, the thermometer being at 0, with 200 grammes of the latter at 25°. The two ethers thus mixed after two days standing indicated 30°.

I distilled about 70 grammes of ether; it indicated 39°, and had the smell of sulphuric ether very perceptibly; whence I concluded, that the mode I had employed was the only one for separating the two ethers.

The two kinds compared.

I afterward subjected the acetic ethers to the following examination.

1. They

1. They were both of a pleasant smell.

2. Their specific gravity differed only four or five degrees.

3. They began to boil at nearly equal temperatures: the first at 50° of Reaumur [144½° F.] the second at 46° [135½°] making a difference of 4°, [9°.]

4. Exposed to the air they evaporated slowly.

5. They were both equally soluble in eight parts and half of water.

6. Sulphuric acid has very little action on these ethers; it colours them slightly; and one part of ether and one of acid, very completely mixed, evolve but little heat, about 30° [67½°.]

7. Nitric acid at 46° is powerfully decomposed by these ethers, a considerable quantity of nitrous gas is evolved, and the residuum is oxalic acid.

From these different facts it follows, that the two ethers are nearly the same, having only some shades of difference, which do not affect their nature.

Thus the process proposed by Mr. Parmentier appears preferable to that of Mr. Pelletier, in being less expensive, requiring a shorter time, and furnishing a larger quantity of ether. But, while I give the preference to this process, I am far from subscribing to the assertion of Mr. Gehlen, that a mineral acid is necessary to the formation of acetic ether.

The process with sulphuric acid preferable,

but not essential.

XIV.

Inquiries concerning the Oxidations of Iron; by Mr. Darso.*

It is ten years since the celebrated chemist, Professor Proust, struck with the two combinations that some metallic oxides form with acids, and reflecting on the two proportions of oxigen, that unmetallic combustibles usually take, advanced the opinion, "that metals combine with oxigen only in two proportions:" and though several chemists have since maintained, that there are intermediary

Proust supposes metals to combine with oxigen in two fixed proportions only.

Others the contrary.

* Journal de Physique, Vol. LXIII. p. 292. October, 1806.

oxides;

oxides; and the author of Chemical Statics has gone still farther, asserting, "that the proportions of oxigen united with metals vary, from the point at which the combination is possible, to that in which it has attained its highest degree;" Prof. Proust has not considered the facts objected to his doctrine as sufficient, and persists in the opinion, that nature has fixed these two invariable terms of oxigenation.

Though I consider the subject somewhat differently from the Madrid professor, I have a high opinion of his labours and observations, and incline to think with him, not that the proportions of oxigen are invariably determined by nature, but that most of the facts, on which the opinion of intermediary oxidations are founded, have not all the accuracy such a discussion requires.

But we have not facts of sufficient accuracy to determine the question.

Persuaded, that every research tending to elucidate this point of theory cannot but be of great utility to the advancement of science, I proposed to myself to make some experiments on iron, as one of the metals best adapted to such researches: and I shall relate them in the order I pursued in my labours, persuaded, that I could not adopt a better arrangement, than that of following the ideas that suggested them.

Iron well adapted to the investigation.

New oxides might be obtained by the aid of compression,

The first means that occurred to me for discovering new oxides of iron were, 1st. to treat the red oxide with oxigenizing substances, confining the expansibility of the oxigen by compression. As experiments of this kind relative to the carbonic acid succeeded so well with Sir James Hall, I had no doubt of thus increasing the oxigenation of iron. 2dly, to subject iron wire to different discharges of electricity in air containing more or less oxigen. Previously however I was desirous of ascertaining how iron comports itself in other modes of treatment, to which it has been already subjected.

or the electrical discharge in oxigen.

Oxides by Calcination.

Iron calcined with nitre,

much passed through the crucible.

I took one part of iron filings and three of nitrat of potash well powdered, mixed them, and threw them into a red-hot crucible. After keeping up the fire for three quarters of an hour, I withdrew the crucible, and found a great part of the potash and oxide of iron had passed through it. The

mixture

mixture when cold exhibited a brown mass, with a few green and iridescent spots.

This mass, pounded and washed repeatedly with boiling water, to divest it of its alkali, afforded me a brown powder, strongly attracted by the magnet, and not soluble in cold muriatic acid. Heated with this acid diluted with a little water, it afforded a colourless solution, from which alkalis precipitated a blackish brown oxide, that did not alter by exposure to the air, and at the expiration of a few minutes, had acquired so great a cohesive force, as to be insoluble in cold muriatic acid. When dried in the air it was magnetic, and indeed retained the same characters as before it was dissolved. *

The oxide brown, magnetic, and not soluble in cold muriatic acid.

Dissolved and precipitated, the same.

As the loss prevented my calculating the quantity of oxigen in this magnetic oxide, while its colour and magnetism led me to conclude, that it contained less than the red, obtained by calcining iron filings alone, I proposed to try this method, seizing the moment when the magnetic oxide should be formed. Accordingly I put into a crucible 100 grains of iron filings, and after having kept them half an hour in a brisk heat, stirring constantly, I withdrew them, and found the weight 120 grains. I observed on this occasion, that each grain of the filings, though covered with a stratum of oxide, contained a metallic nucleus; and in order to expose the metal, and accelerate the operation, I triturated these half oxided filings, before I put them again on the fire. On continuing the calcination, and trying the oxide occasionally with muriatic acid, I found the magnetic

Iron filings calcined alone.

* I fancied this at the time to be a peculiar oxide; but I afterward perceived, that its colour and magnetism arose from the concentrated state of the solution and of the alkali with which I precipitated it; since if I diluted the alkali with water, or used lime, strontian, or barytes water, the precipitates were entirely red.

Cause of the colour and magnetism.

On mixing green and red solutions of iron in a certain proportion that may be found by trial, we likewise obtain black magnetic precipitates, that do not change on exposure to the air: but the two phenomena must not be confounded together, for there are magnetic oxides, that do not contain an atom of green oxide. The green salts of iron too may be precipitated so as to be black, magnetic, and unchangeable by the air.

Other magnetic oxides.

Magnetic from ·28 to ·265 of oxigen.

oxide succeed the green, when the 100 grains had absorbed from 30 to 36 of oxigen.

This experiment, which I repeated several times, always afforded me the same results, except that sometimes I found a few hundredth parts of red or of green oxide. It is obviously impossible however, from various circumstances, that every particle of the iron should be equally exposed to the action of the air and the caloric.

Sometimes a little red or green oxide with it.

Red and not magnetic with ·284.

Oxigenation rather slow,

but carried as far as ·56 nearly.

On carrying the calcination so fair, that 100 grains had taken up 38 of oxigen, the precipitates were entirely red, without exhibiting any trace of magnetic oxide. From this term to that of 45 or 50 the oxigenation was very slow, and would have been impracticable without an increase of heat: by raising the fire however, and renewing the air by means of a pair of bellows, I carried the oxigenation as far as 56. This operation is very tedious and tiresome; but an apparatus to save the trouble of renewing the air might easily be contrived, if the excess of oxigen were of any advantage in physic or the arts.

All the oxides after the magnetic alike.

I treated this oxide at 56 with acids, and afterward precipitated it by the alkalis and alkaline earths; I likewise added to its solutions prussiates, gallates, and phosphates; to see if I could discover any properties distinguishing it from the oxides that had preceded it. My trials however were in vain, as I might have expected, since those at 38, 40, 45, and 48 had afforded me no characters to distinguish them from one another.

Redness and magnetism not owing to proportion of oxigen.

The only difference I observed among these oxides was, that the red colour became more decided, and the magnetism weaker, in proportion as the oxidation advanced: but these properties depend more on the difference of the compactness or density of the oxides, than on their proportion of oxigen. *

Different salts of a metal not owing to difference of the oxide.

This conformity of the properties of oxides, among which the difference of the proportion of oxigen extends as far as 20, or even 40 hundredths, as I shall show, proves the error of requiring the formation of a different salt as characteristic of each degree of oxigenation. In fact too extensive an influence on the oxides of iron, and I believe on all the me-

* See the Note subjoined at the end of this Memoir.

tallic

tallic oxides, has been ascribed to oxigen. It has been supposed, that all the properties of oxides of iron, both chemical and physical, are owing to oxigen; while on the contrary, from my observations I am induced to believe, that its part is so passive, as to give it a claim to scarcely any of those properties.

Oxides variable under similar circumstances.

Notwithstanding the uniformity of the circumstances, to which I had subjected the iron in its different calcinations, I observed, that, when 100 grains had taken up 28 of oxigen, the oxide was sometimes entirely magnetic; while at other times, having taken 30 or 32, it afforded a very deep and extremely homogeneous green precipitate; and lastly that at times the red oxide occurred by anticipation at 28 or 30. As I was prejudiced in favour of the less or greater divisibility of substances, on which I believe most phenomena depend, I did not hesitate to ascribe to this cause the results I had obtained: but to satisfy myself on this head, I took 300 grains of iron filings of three different densities, and each portion less dense than that I had before employed. Let us suppose the ratio of their densities to have been at 1, 2, 3. I subjected to calcination the 100 grains of the most dense, removing them from the fire every ten minutes, to triturate them in a mortar. At the end of half an hour, and having been triturated three times, they had taken up 24.70 of oxigen, their colour was become altogether red, their magnetism was very weak, and dissolved in muriatic acid they afforded red precipitates like oxide at 56. I repeated the same experiment on 100 grains of the second degree of density, and when they had taken up 21 of oxigen, the oxide displayed the same properties as the preceding. Finally I subjected to the same proof the last 100 grains, which were extremely fine, and which I had previously sifted, that their density might be more uniform; but instead of taking them from the fire every ten minutes, I triturated them every five, to diminish the action of the oxigen as much as possible. In the space of a quarter of an hour they had taken up 15 of oxigen, and the properties of the oxide were the same as those of the two preceding*.

Most phenomena depend on the divisibility of substances.

Experiments with filings of different densities.

Oxides similar

Thus

* Sometimes they contain 2 or 3 per cent of green oxide, which is not perceived, and which it is difficult to separate, even though it

Red oxide with 15 of oxigen:

Thus we have a red oxide of iron made in fifteen minutes, which gives a fine blue with alkaline prussiates, is precipitated black, or rather of a very deep blue, by galls, and is not distinguishable from the oxide at 56, at least by any of the means hitherto employed for this purpose.

Proportion of oxigen therefore variable.

All these facts prove the sagacity of the learned author of the Statics, when he says, not from elective attractions, but from the properties of oxigen and metals, that the proportion of oxigen to metal may vary from the point at which the combination is possible, to that in which it has attained its highest degree, and that a multitude of circumstances may check or increase the proportion.

I did not carry this experiment farther, but I conceive, that by favouring the division of the iron by all possible means, and at the same time opposing obstacles to the action of the oxigen, we might obtain red oxides of iron with only 6 or 8 per cent of oxigen. And who knows whether by preventing the action of this principle altogether, we might not obtain powders of iron soluble without effervescence in acids, and enjoying the same properties as oxides? For my part I am the more persuaded of it, as I pay little regard to the principle generally received, that metals must be previously combined with oxigen before they will unite with acids. I consider the oxidation rather as a consequence of the means we employ to divide the metals, and reduce them to the degree of fineness required for their solution in acids, than as an indispensable condition of their solution. On this subject I intend to make some researches, and I may then explain the motives that oblige me to question this principle.

Perhaps red oxides with 6 or 8,

and iron soluble in acids without being oxided.

Other metals the same.

To what are the medical virtues of these oxides owing?

Before finishing my report concerning the oxides of iron by calcination, I would wish to make one remark respecting their most interesting application, their medicinal use. It is not yet known, whether these owe their virtues to the iron or to the oxigen; and as the proportions of these in the various preparations of this metal are undetermined, we are wholly ignorant which deserves the preference: this therefore is an object, that merits a careful investigation.

(To be continued.)

it is known to be present. Digestion for half an hour however in very dilute muriatic acid will dissolve it, or rather the iron it contains, without attacking the red oxide.

SCIENTIFIC

SCIENTIFIC NEWS.

Mr. Jessop's *Method of blasting Rocks.*

THE information respecting the blasting of rocks given in our Journal, vol. IX. p. 230*, has not only been conveyed to France, but the process has been followed there, and different experiments made on it. Mr. Baduel, an engineer employed in executing that part of the road from Simplon, which extends along the south shore of the Lake of Geneva, has availed himself of it on that occasion. He used the common charge of powder there, which is sufficient to fill one third of the hole, and at first filled the rest of the hole with sand. This quantity of sand he diminished gradually, till he found, that two thirds as much as the powder were sufficient. Bran, saw-dust, ashes, and other light pulverulent substances, substituted instead of sand, produced the same effect.

Rocks blasted with sand in Switzerland.

Bran, saw-dust, ashes, &c answered likewise.

Several mines thus charged, and made with various degrees of inclination, in single blocks of stone, and knotty trunks of trees, succeeded as completely, as if they had been stemmed with the greatest care. But the success was not so uniform in the mass of the mountain itself, composed of a blackish siliciferous limestone. In this case the explosion frequently took place without affecting the rock, though the hole was sometimes filled with gunpowder two thirds, or even three fourths of its height †

Failed in the solid rock of the mountain.

These results are scarcely consistent with those obtained by Mr. de Candolle, in the works carrying on over Mount Cenis. This gentleman has seen the blasting with sand executed with success repeatedly, not in separate blocks, but in immense rocks of a micaceous lime-stone schist. Some of these mines, charged with two ounces of powder only, produced as much effect as if they had had the usual charge, which is double that quantity.

Succeeded however on mount Cenis, with a small charge of powder.

* See also, Vol. XII. p. 60.

† The failure in these instances may obviously be ascribed to so large a portion of the hole being filled with gunpowder, the sand being forced through the remaining short space, before the resistance offered by the solid rock could be overcome. The next paragraph confirms this. W. N.

This

At Pesey it succeeded in separate blocks, but not in the solid rock.

This method has likewise been tried at the mines of Pesey. On a separate block it was completely successful; but it was not so in the solid rock. Eight or ten holes 3 cent. (11·8 lines) in diameter, and 3 or 4 dec. (12 or 15 inches) deep, were bored at the same time in the vein itself. Some were charged in the old method, others in the new, putting into each the quantity of powder judged to be sufficient by the workman himself and varying from 6 to 9 decigrammes (2 to 3 oz). All the holes stemmed in the old way blew up the rock; those covered with sand did not even split it.—Some of the latter were charged in the same way with a double or triple quantity of powder, so that it occupied half or two thirds of the hole, the remainder being filled with sand; and the explosion again took place without any effect on the rock. The same holes, which had resisted this double trial, being charged again in the old way with the usual quantity of powder, produced a complete fracture.

Perhaps the effect of this method limited.

From these trials we should be led to conclude, that the new process, though very good for blasting rocks, will not always succeed, when applied to the solid rock of a mountain, which generally presents itself bare but in part; and still less in the interior parts of mines, where the points of contact are more numerous. There seems to be a limit of resistance, beyond which we cannot go by the new method, as we may by the old.

An improvement by Mr. Pictet.

Professor Pictet has proposed another improvement. It is well known, that, in military operations, by making the capacity of the chamber of the mine equal to four times the bulk of the powder employed, a less concentrated explosion is produced, but more destructive at a distance, than if the wadding were in contact with the powder itself.—Muskets too, and cannons are daily burst by leaving a space between the powder and the wadding. Now in working mines, this lateral explosion is exactly what is wanted as strong as possible; it is probable therefore, that it might be obtained with less powder, by leaving an empty space between the wadding and the powder. This method is said to have saved several thousand crowns annually, by diminishing the consumption of powder to that amount, in the mines of the Hartz.

Leaving a vacuity over the powder.

Great saving by this in the Hartz.

Mr.

Mr. Pictet accordingly recommends to those who are engaged in mining a combination of the two methods; one of which, the blasting with sand, would give security to the miners; the other, a partial vacuity, would save powder. This may easily be effected, by introducing into the hole, after the powder, a cylinder or cartridge of paper, open at one end, and with a small hole in the other, which is to be placed uppermost, to admit the priming straw. Over this the sand may be poured in, and thus a vacuum of two or three inches between it and the powder preserved. His method.

Mr. Gillet-Laumont suggests another additional contrivance. He thinks, if the hole be vertical, or not much inclined from this direction, a more forcible concussion might be given to the rock, by loading the sand with a heavy weight. To effect this he would introduce into the hole an iron cylinder, with a lateral groove for the passage of the straw, and surmounted by a heavy mass of iron, being a continuation of the cylinder. This he supposes would add greatly to the resistance; and the same piece of iron would serve repeatedly for the same purpose, as it could not be blown far, and therefore would easily be found. Gillet-Laumont's improvement. A heavy weight over the sand.

Extract of a Letter from Mr. Gehlen *to* J. C. Delametherie.*

SIR,

YOU are acquainted with the observations of Sir James Hall on the effects of heat modified by compression †; but Mr. Bucholz has just written to me, that powdered chalk may be converted into a substance analogous to marble without compression.

Wanting to prepare some quicklime, he put four pounds and a half of pure washed chalk into a Hessian crucible, which he covered with a brick, and exposed it in a wind furnace for an hour to a bright red heat, not gradually raised. On examining the contents of the crucible, Mr. Chalk fused without losing its acid.

* Journal de Physique, Vol. LXIII. p. 238, Sept. 1806.

† See Journal, Vol. IX. p. 98; XIII. p. 328, 381; and XIV. p. 13, 113, 196, 302, 314.

Bucholz found it contracted one sixth. The chalk on the surface and next the sides of the crucible was quicklime to the depth of a line; but this was followed almost to the centre by laminæ adhering strongly to each other; very hard and solid, half fused, and of a yellowish white colour, with a reddish tinge scarcely perceptible. Their hardness was so great, that here and there they would scratch glass; and their softening, or incomplete fusion, which had taken place, was very evident, notwithstanding their laminar form. Under this schistose mass was another, extending to the bottom of the crucible, which bore still more decided marks of fusion. It was broken into seven or eight pieces, which exhibited a perfectly smooth, flattened, conchoid fracture; were so hard in some parts on their edges, as to cut glass, and so solid, as to require a pretty stout stroke with a hammer to break them. Small fragments were in some degree or even quite transparent.

Only one per cent. of its acid expelled.

On dissolving this fused chalk in muriatic acid, it lost 0·42 of carbonic acid, of which before it was heated it gave out 0·43. The acid exhibited itself with all its characteristic properties, and had not undergone the least alteration.

Magnetic iron stone containing red oxide.

Mr. Bucholz has shewn too, that the magnetic iron stone of Suhl in Germany, is iron at a maximum of oxidation, or in the state of red oxide. This appears singular, as it has hitherto been supposed, that the magnetic property is confined to the black oxide, and is destroyed by an excess of oxigen; as it is according to Mr. Hatchett by an excess of sulphur and of carbon, and perhaps of phosphorus.

Volcanic calcedony silex alone.

The same chemist has analysed the hyalite of Frankfort, or volcanic calcedony, and found in it nothing but silex. He had a loss however of 7 per cent. probably therefore it contained also an alkali, which he was not able to examine into, for want of a larger quantity.

Chromat of iron.

Mr. Klaproth has analysed a new fossil from Virieglach in Stiria, which afforded chromat of iron, mixed with a foliated talc in curved laminæ. It was tinged of a cochineal red, and a peach-blossom colour by the chrome.

Flint of recent formation.

Mr. Haquet, of Cracow, the author of several geological works in much esteem, has communicated to me a memoir on the formation of gun flints, and the different

situations

situations in which they are found. He thinks them of very recent origin, since they occur only in calcareous mountains of secondary formation, and near their surface; and besides he has found in these mountains petrified roots, wood, and animal substances. In several fragments he has met with rhomboidal crystals passing in gradation from carbonate of lime to nearly pure silex.

Muriatic acid and soda formed by galvanism.

You will find likewise in my journal a paper by Mr. Ritter, concerning the muriatic acid and soda formed by the two poles of the pile. He is pursuing his experiments on this subject; and Messrs. Berzelius and Hisinger are doing the same, for they observed the formation of these in 1802, and consequently before Pacchiani.

Autographs from Stone Blocks.

Printing from autographs on stone.

A method of printing from designs made on stone was mentioned in the last volume of our Journal, p. 158. I am informed, that the circular letters from a snuff manufactory at Offenbach to its correspondents, are printed in this manner; and that a Mr. Reuter, a painter, of Berlin, was the inventor.

Art of Swimming.

Swimming society in Denmark.

A society has been formed in Denmark for improving and extending the knowledge of the art of swimming; an art certainly of great utility with respect to health, cleanliness, and safety, and particularly valuable to a maritime nation.

Description and Chart of the Faro Islands.

Chart and description of the Faro islands.

Mr. Loevenoern, a distinguished officer in the Danish navy, has lately published a new chart of the Faro islands. A particular and interesting description of this little known portion of the Danish dominions is given with it.

Scientific

Scientific Voyage.

Scientific expedition from Russia.

AN expedition has been fitted out from Kamtschatka to the Curile and Aleutian islands, and the North West coast of America, the objects of which are entirely scientific. Mr. Redowski, who accompanied the embassy from the court of Russia to Pekin, as botanist, is placed at the head of it. An astronomer will sail with him for the purpose of making observations, but his name is not mentioned, or that of any other man of science. The voyage is to be of three years duration.

Rockets discharged by Electricity.

Application of electricity to discharge cannon.

THE 14th of February, at two o'clock in the afternoon, M. Bouche made an experiment in the *Jardin des Plantes* at Paris, to try the effect of electricity applied to gun batteries. Instead of guns he had fixed about one hundred rockets on long sticks, disposed in the garden. The rockets were all connected by an iron wire, and the same spark caused them all to explode at the very same instant. The concourse of people was very great, the weather being remarkably fine. This new invention is not intended to increase the destructive powers of those formidable weapons; but it is expected to afford the means of using them without exposing gunners to the fire of the enemy.

Imperfections of Alegar.

Premium for the improvement of alegar.

VINEGAR made of beer, properly called *alegar*, constantly retains a mucous matter, which prevents it from *keeping*. The society of amateurs of sciences and arts at Lisle, wish to have this defect removed; and propose a medal for the best mode of improving alegar in those qualities which may render it equal, or nearly so, to the best wine vinegar. It deserves notice, that this liquor has some properties, which, could they be separated from others not so valuable, would render it worthy of more attention than it has hitherto received.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1807.

ARTICLE I.

A Memoir on two new Classes of Galvanic Conductors. By Mr. ERMAN*.

THE faculty of propagating or isolating electric effects, exhibited in such different and variable degrees by different substances, eminently demands our attention, because the time is arrived for comparing this faculty with the chemical constitution of bodies, to establish something respecting the nature of the electric fluid. The anomalies of the conducting faculty are so strongly marked in galvanic electricity, that they have afforded arguments to those, who refer the phenomena of this class to a principle essentially different from electricity.

Galvanism has been supposed different from electricity, from anomalies in the properties of conducting substances.

The examination to which I have subjected a great number of substances, with respect to the phenomena they present, when they are employed to complete the galvanic circuit from one pole of the pile to the other, has furnished

New experiments answer some of the arguments,

* Journal de Physique, Vol. LXIV. p. 121. Feb. 1807.

To this Memoir the French National Institute awarded the Prize of 3000 fr. [£125], founded by the Emperor, to be given annually to the best paper on the subject of Galvanism, till a discovery on its principle or application shall be made, of sufficient importance to merit the sum of 60,000 fr. [£2500], of which this is the interest.

me with answers to some of these arguments: but I have obtained a result of much more importance, since I have convinced myself by authentic facts, that in effects of this kind every possible combination is realized; for, if any substance be applied to the two poles of the pile, one of the five following effects will take place.

and lead to a new arrangement of conductors.

1. Perfect nonconductors.

1. Either this substance, not acting separately on either of the two poles, leaves them perfectly insulated, when we attempt to set them in action by its intervention. The result of this perfect insulation is, that the galvanic circuit is not completed; and that the electric tension remains at its natural maximum at each pole, without our being able to modify it by the interposition of the substance employed. *Perfect nonconductors* are cold glass, oils, and resins, in every state of aggregation; water, when solid or in vapour; &c.

2. Perfect conductors.

2. Or the two poles exert, through the intervention of the substance applied, a reciprocal action so intimate, that, perperfectly neutralizing each other, every phenomenon peculiar to each ceases, so that it is impossible to act in a distinct and appreciable manner on either of them. *Perfect conductors* are all metals without exception, and in the same degree, at least as far as we know: for it must be observed, that it is only from analogy we ascribe this property to those that have not actually been subjected to experiment; and it is possible, that some metal may have exclusive properties with respect to galvanism, analogous perhaps to those of magnetism and iron. The possibility of this, and the great importance of the discovery, demand a series of experiments, from which we ought not to be deterred by the little probability there is of success.

3. Imperfect conductors.

3. Or the substance applied to the two poles permits their reciprocal action, and completes the galvanic circuit, but in such an imperfect manner, that the distinct effect of each pole will continue to manifest itself, and that it will be possible, by the intervention of the substance applied, to influence each pole separately, according as we act on one extremity of the imperfect conductor or on the other. This property, which I have demonstrated in moist conductors, and in water itself, is so much the more important to be studied,

studied, as it is connected with chemical and physiological phenomena. In fact, except in the case of sparks alone, there is no decomposition that takes place but in conductors of this class; and all the parts of organized bodies, that galvanic electricity is capable of modifying, equally belong to it.

4. Or the given substance, acting as a perfect conductor when applied separately to either of the two poles, is found nevertheless to belong exclusively to the positive pole, as soon as it is applied to both at once to complete the galvanic circuit. Conductors of this kind do not close the circle completely from their insulating the negative effect; and in the contact of the two poles by their interposition, we can neither charge the positive, nor discharge the negative pole. 4. Positive conductors.

5. Or lastly, the effect mentioned in the preceding paragraph is inverted, that is to say, the substance, that acts on either pole separately as a perfect conductor, belongs entirely to the negative pole, as soon as it is applied simultaneously to the two extremities of the pile. Hence results a maximum of electric tension in the positive pole, and the impossibility of producing any divergence at the negative side by the intervention of substances of this class. 5. Negative conductors.

The phenomena of the first and second class have been known too long to excite our attention, though they furnish many interesting particulars. Those of the third I suppose to be equally known; and therefore I shall confine myself to the facts, that demonstrate the existence of conductors of the fourth and fifth classes. These facts, beside their novelty, afford some interesting problems to be solved, and new views to be pursued in galvanic researches. Phenomena of the 1st, 2d, and 3d, already known.

Before I proceed to the new facts I have to offer, I shall observe, that the phenomena in question do not exhibit themselves with clearness, and in their whole connexion, but as far as they are studied with the assistance of the electrometer alone applied directly to each pole; and without having recourse to the condenser, the employment of which being always interrupted, and its language frequently equivocal, sometimes even deceitful, it fetters the progress of the observation, and never allows us to take it in at one view Must be examined by the electrometer alone. Inconveniences of the condenser.

all the changes, that characterise each state of the pile. In the nice experiments I have to relate, it will be seen, that the number of simultaneous observations to be made would render the use of the condenser extremely inconvenient: and if the modifications necessarily produced at each pole by the augmentation of the electric capacity, that results from the very application of the condenser, be considered, the reason of my excluding it will be obvious. Gold leaf electrometers, applied immediately to the poles, and to the the subjects of the experiments, are free from every inconvenience; and if they be ever so little sensible, they indicate with extreme fidelity and promptness the progress and degree of the most complicated modifications, that the pile undergoes.

The gold leaf electrometer very convenient.

Another essential condition to the success of the investigation is, that the pile and all parts of the apparatus be perfectly insulated. I have found no mode of insulating the pile better than to fix it in the centre of a large cake of resin, taking care not to render the cake an electrophorus by any accidental friction. As to the other parts of the apparatus, we should never trust to the insulating power of glass alone; and in applying a resinous coating to the surfaces, I have found the dry way far preferable to the moist. Lastly, before commencing the experiments, and during their course, it is proper to try by means that may readily be contrived and varied, whether all parts of the apparatus completely insulate the electric effects: and it is particularly important, to pay this attention to the electrometers, to be certain whether the glass of these instruments, which cannot be coated with resin, preserve itself constantly in a perfectly insulating state. I know by experience, that the progress of the observations is frequently confused, from the surface of the electrometer having imperceptibly become a conductor. This inconvenience is remedied by drying the instrument, and not by exhausting it, for fear of falling into a still worse inconvenience, the communicating to the glass an electric charge.

The apparatus must be perfectly insulated.

Glass not sufficient.

Particular attention to the electrometer necessary.

Sect. I.

Of Conductors, that, in establishing a Contact between the two Poles, insulate the negative effect, while they continue to propagate the positive Electricity.

When we apply separately to each of the poles of the pile the flame of a spirit lamp, it acts as a perfect conductor: but if it be applied simultaneously to both poles, it completely insulates the negative effect, while it continues to conduct the positive electricity with the same energy; and in consequence of this partial insulation, the electric circuit is not completely established.

Flame a perfect conductor to either pole separately, but insulates the negative electricity when applied to both at once.

The faculty that flame possesses of conducting the fluid of the pile, which has been so much disputed, is placed beyond doubt by the following facts.

Its conducting power disputed

To either of the poles of a perfectly insulated pile of a hundred pair of plates, more or less, apply a very sensible gold leaf electrometer, which will presently acquire the degree of divergence corresponding to the energy of the pile, and the more or less perfect insulation of the opposite extremity by the circumambient air. As soon as the divergence of the instrument is become stationary, present to the metallic wire of the opposite pole the flame of a spirit lamp completely insulated; and the divergence of the electrometer will not be increased. But the moment a communication is established between the flame and the ground, by introducing into it a wire not insulated, the electrometer will diverge as much as if a communication had been established between the opposite pole and the ground, by means of an uninterrupted metallic conductor. This effect is the same at the negative as at the positive pole, a circumstance which will appear by and by of importance. Electricity therefore may be communicated to either of the two poles of the pile by the medium of the flame of spirit of wine.

Facts that prove it.

Electricity communicated by flame to either pole.

In the same manner it may be radically abstracted from either of them. Let each pole communicate with an electrometer by means of a wire. If an insulated flame touch either of these wires, the corresponding electrometer will lose

Abstracted by flame from either pole.

lose nothing of its divergence; but it will be completely deprived of it, the moment a direct communication between the ground and the flame is established.

Both effects may be shown at once by two flames.

These two effects may be seen at once acting in combination, by preparing two perfectly insulated flames, and guiding into each one of the wires proceeding from the two extremities of the pile. If the insulation be perfect in all points, both the electrometers will indicate after a few seconds the same state of divergence, as if the poles were not in contact with the flame. Now if one of the flames be made to communicate with the ground, the electrometer of that pole will immediately lose all its divergence, and the divergence of the electrometer of the other pole will be a maximum. The alternate contact of the two flames therefore produces the same effect, as if we had immediately touched the extremities of the pile itself.

Farther proofs of the conducting power of flame.

Lastly, that we may be fully convinced of flame being an excellent conductor for all the effects of the pile, that do not depend on the closing of the circle, the following facts should be noticed.

Bend the wire on the top of the electrometer, so that the point shall terminate in an insulated flame. Into the same flame insert a wire from one of the poles. If now the opposite pole be touched, the electrometer will receive a maximum of divergence corresponding to the case. If afterward the electrometer itself be touched, the pole with which it communicates through the medium of the flame will be discharged. Lastly, by touching the flame, we shall discharge at once both the electrometer of this pole, and the electrometer communicating with the flame.

Flame therefore does not insulate galvanism, and conduct electricity

These facts prove to a demonstration, that the flame is far from insulating the electric effects of the pile in the cases indicated. They show, that with respect to these cases there is no ground for admitting a galvanic fluid, which the flame insulates, in opposition to the electric fluid, of which it serves as a conductor.

Its conducting power inferior to that of metals.

In the following fact, however, we find an anomaly, which shows us, that the conducting power of the flame, however perfect it has appeared to us in the preceding experiments, is nevertheless very inferior to that of metals, when these

two

two kinds of conductors act in opposite directions. If one of the poles communicate with an electrometer by means of a wire, an uninsulated flame, brought into contact with this wire, will take from it, as has been seen, all the divergence before imparted to the electrometer by the *transient* contact of the opposite pole. But if a *permanent* metallic communication be established between this pole and the ground, the electrometer will reach the maximum of divergence, and remain at it without any diminution, though the uninsulated flame continue to touch the wire, by means of which the electrometer is in communication with the pile. It is to be observed, that this effect is precisely the same at both poles. But how different would be the action of a metallic conductor, if in this experiment it were substituted instead of the flame! It is well known, that the application of uninsulated metal would prevent any intensity of electricity producing divergence; and that the application of a humid conductor would at least diminish it extremely, if it did not reduce it to nothing. Flame therefore, which has hitherto been considered as a good conductor, does not here produce the effect, that was to be expected from it.

It conducts positive electricity, and insulates negative, at the same time.

But this anomaly is of little importance, compared with that which flame exhibits, when it is applied simultaneously to both poles, with a view to close by its means the galvanic circle. The following facts prove, that in this case it belongs entirely to the positive pole, and absolutely insulates all the negative effects, which has led me to place it in a separate class.

Experiment in which it appears a nonductor.

Let each pole of a well insulated pile, consisting of about a hundred and fifty pair of plates of silver and zinc, be connected with a sensible electrometer. With each pole connect a wire, supported by a completely insulating stand; and let the extremities of the wires be brought so near together, that one flame may be in contact with both. On an insulating stand place a spirit lamp, and commence the experiment by bringing the flame into contact with the two metallic wires. As long as the flame remains insulated, the electrometers of both poles will diverge nearly as if the two polar wires were perfectly insulated. After some time, indeed, the electrometer of the negative pole will exhibit a little

stronger

stronger divergence than that of the positive, though every thing else will appear to indicate an absolute insulation: for if a communication be established between either of the poles and the ground, its electrometer will lose all its divergence, and that of the opposite pole will attain its maximum; and on touching both poles at the same time, as strong a shock will be received, as if the two poles were insulated by a stratum of air. It appears, that hitherto philosophers have contented themselves with this single experiment, to affirm that flame insulates all galvanic effects; but the following facts prove, that this insulation is partial, and that flame continues to be an excellent conductor for the positive pole.

Proof that it conducts positive electricity.

Every thing remaining as in the preceding experiment, let a communication be made between the flame and its support; or, which is more simple, touch the flame itself with an uninsulated metallic rod. Immediately all the divergence passes to the negative pole, and the positive is absolutely discharged. If the strongest divergence possible have been previously given to the negative electrometer, by touching the opposite pole, no application of a good conductor to the flame will take off the least part of this negative divergence; while the same application will instantly destroy every vestige of divergence before imparted to the positive pole, and transfer it to the negative side in the strongest degree possible.

Whatever extent be given to the flame, and however near to the negative wire it be touched, it still remains impossible to act through its medium on the negative side, so as to take away the divergence. Flame belongs wholly therefore to the positive pole, since by touching it this pole is immediately discharged, and the negative pole is mediately brought to a maximum of divergence.

This farther confirmed by experiments.

This paradoxical property is confirmed by the following experiments. The two polar wires being united in the same insulated flame, immerse in this flame the hook of a sensible electrometer, and it will acquire a weak positive divergence, if the two poles had not previously arrived at an equilibrium of intensity. But this positive divergence attains its maximum, the moment the negative pole is made

to

to communicate with the ground. If we afterward touch the positive pole, the electrometer immersed in the flame immediately loses all its divergence. Lastly, if a communication be established between the ground and the flame itself, both the electrometer in contact with it, and that applied to the positive pole, are discharged, while that on the negative side attains the highest degree of divergence. These effects are completely explicable, on the supposition that the negative pole is insulated in the flame, while to the positive it is a conductor.

Flame gives out positive electricity to a distance of 1½ or 2 feet perpendicularly,

What renders this absolute insulation of the negative pole by a conducting substance still more paradoxical is the very intimate relation the flame bears to positive electricity. In fact, to take from this pole the divergenee that has been given to it, it is not necessary actually to touch the flame; it is sufficient to bring over it, at the distance of a foot and half, or even two feet, a metallic conductor communicating with the ground; when the positive electrometer will immediately arrive at zero, and that of the negative at the maximum of electric intensity. In like manner an electrometer, the hook of which is held at a similar distance above the flame in which the two polar wires of a powerful pile terminate, will very readily become charged with positive electricity, when a communication is made between the ground and the negative side, and will be discharged on touching either the flame, or the pole of which that flame so eminently propagates the effect. This action of the flame extends laterally also, but by no means with equal energy, for in this direction it is confined to a few inches.

and to a few inches horizontally.

Flame does not complete the galvanic circle,

All the indications by the electrometer, that have been related, prove, that the galvanic circle is not completed by the intervention of the flame; and experience long ago showed, that the decomposition of water did not take place, and the physiological effects of the pile were not manifested, when the exciting arc was interrupted by the interposition of flame. Reflecting, however, on the faculty flame has of conducting the electricity of each pole separately, and insulating only the negative effect, it appeared to me possible, to obtain some momentary effects on animals, by discharging at once into the ground the two poles united by the flame, and

yet momentary effects on the nerves may be produced through it.

and placing very irritable organs in the way of the discharge. After several fruitless attempts, I arrived at the following combination, the success of which has never since disappointed me, and perhaps furnishes an interesting datum for the general theory of the electric charge.

Frequently attempted in vain.

Successful experiment.

Let a powerful pile be perfectly insulated, and its two flames be united in one insulated as perfectly. Prepare as speedily as possible the hinder extremities of a frog, so that the ischiatic nerves shall be disengaged from the flesh, and from the spine, the lumbar vertebræ of which are removed. Place the muscles on the negative pole of the pile, letting the nerves hang down freely; and, holding an exciting arc by a completely insulating handle, apply one extremity to the flame, and the other to the nerves. By this no contraction will be occasioned: or should there by chance be some traces of contraction, as in fact has occurred to me, though very rarely, these must be considered as exceptions produced either by the defective insulation of the handle, a mere mechanical irritation of the very susceptible nerves, or by the action of the atmospheres of the poles; for I have found in another series of experiments, that every pole, charged by the contact of the opposite pole, becomes the centre of a sphere of activity, in which the capacity of substances is powerfully modified without contact, and solely by the mechanism of electric influences.

Cautions.

This may account for sparks being obtained by the contact of a single pole.

I am tempted to explain by the last mentioned property those sparks, which observers of credit affirm they have obtained by the contact of a single pole, when the pile consisting of a thousand pairs possessed very great energy: and I conceive, that the contractions sometimes seen in the case in question result from the weak positive electrisation, which the negative pole produces by its influence on the exciting arc, so that the equilibrium is restored not between the positive and negative poles, to which the flame presents an insurmountable obstacle, but between the negative pole and the anterior part of the insulated arc, become positive by influence. It is obvious, that the effect of this restoration of equilibrium must be of infinitely small intensity; and that, to produce the weakest contractions, it supposes an extraordinary degree of excitability.

Be

Be this as it may, to prevent any mistake from creeping in, if the contact of the insulated conductor, which terminates at one extremity in the flame, and at the other at the nerves, produce a contraction during the period of the highest irritability, a few moments should be suffered to elapse, the application of the insulated exciter should be repeated from time to time, and very soon the application will produce no effect. The experiment then properly commences. In fact, when the insulated exciter has no physiological action, it is sufficient to establish a communication between it and the ground, either by touching it with the finger, or taking it in the hand without the insulating handle, and very strong contractions will be produced every time the circuit is completed from the flame to the nerves. The influence of the ground may be proved, by completing the circuit with an insulated and an uninsulated arc alternately. If a certain interval be allowed between these comparative applications, those with the insulated arc will never produce any effect, those with the uninsulated will constantly excite contractions.

Farther precautions.

Experiment.

I must observe, however, that this kind of galvanic excitement, by the intervention of flame and the ground, requires a much greater excitability in the subject, than the common method of completing the circuit immediately from pole to pole; for the muscles are obedient to the latter, long after they have ceased to contract by the application of an uninsulated conductor to the flame. It is to be understood, however, that, if the prepared muscles be placed on the positive pole, and the circuit then be completed from the flame to the nerves, no effect will be obtained, whether the arc be insulated or uninsulated; for, as the flame belongs exclusively to the positive pole, it is obvious, that it cannot produce contractions with the pole of its own nature.

The irritability of the subject must be great.

When placed on the negative pole, no effect is produced.

The explanation of this fact appears to me to arise naturally from what has been said. The flame insulates all negative effect, and consequently cannot complete the galvanic circuit. But in the application of the uninsulated arc between the flame and the nerves, it is properly the ground that serves as an intermediate chain, and the mind may distinguish three different effects at the same instant of time. The first is that of charging the negative pole to a maximum

The fact explained.

at

at the expense of the ground: the second, the returning into the ground all the excess; by which the positive would arrive at a maximum of intensity, were there not a want of insulation: and from this want of insulation results, as a third effect, the momentary discharge of the two poles into the ground. It may be conceived, that very irritable organs, serving as a vehicle to this process, will experience that kind of shock, which accompanies the prompt restitutions of the electric equilibrium. If my object were at the present moment to display a theory of the electric charge, I certainly should not content myself with these germes of ideas, which however appear to me fertile in their consequences. It may be presumed too, that this kind of excitation, in which the ground at large concurs, must require a much greater degree of irritability, than those in which the equilibrium is established immediately from one pole to the other. Whether the impossibility of obtaining chemical decompositions by the intervention of flame depend on this circumstance, I cannot say; or even whether the impossibility be absolute: all I know is, that I have never produced any such effect, notwithstanding the numerous combinations I have tried.

The author could never obtain any chemical decompositions in this way.

When the insulating power, which has been so peremptorily ascribed to flame, be considered, the following observation will appear interesting. To produce the contractions just mentioned, it is not necessary, that the uninsulated exciting arc should immediately touch the flame, as it may be held several inches above it. I have sometimes succeeded in producing contractions, when it has been held a foot and half above it, particularly when I have armed this extremity of the arc with a metallic disc a few inches in diameter, in order to bring it into more intimate contact with the hot air issuing from the flame, and serving as a conductor to the positive electricity.

In this case too the flame acts at a distance.

I shall just mention here another observation, which I have repeated several times, but the particulars of which I am far from having sufficiently studied. When the exciting arc, brought into communication with the ground, has produced a contraction, by being placed simultaneously in contact with the flame and nerves, it will retain this property for about

The exciting arc retains its property some seconds.

about twenty seconds, without its being necessary to keep it insulated during this time. In this state it produces a fresh contraction on touching the nerves alone, without requiring the flame to touch the other extremity. This observation has nothing in it of novelty, as there are many analogous facts: yet it is in some degree interesting, as it facilitates our varying the modes of experimenting. But what induced me to mention it here was, that the success of the experiments, in which an insulated and an uninsulated arc are alternately employed, depends on this circumstance; and for this reason, in describing these experiments, I mentioned the necessity of allowing a certain time to elapse between each of these comparative applications.

Flame of alcohol thus shown to be an excellent conductor of either electricity singly, but to insulate the negative when employed to complete the circuit.

The facts I have recited incontrovertibly prove, that the flame of spirit of wine is an excellent conductor for either pole of the pile; but that in connecting the two poles it completely insulates the negative side, while it continues to be eminently conducting for the positive. But the problem is still far from being solved: it remains to be known, what is the mechanism of the action, on which this singular property depends. It would certainly be rash to determine any thing respecting facts so new, and deviating so widely from all known analogy: I only mention the following hypothesis, therefore, on account of the interesting facts of which I have obtained a knowledge, taking it as a text for farther researches.

Flame supposed in some instances to dissipate electricity: in others to collect it.

I had long imagined, that the electric intensity manifested exclusively at the negative pole by the intervention of the flame might depend on the two opposite properties assigned to it, and in fact distinguished in certain phenomena of common electricity. We conceive we have equally reason to say, that flame dissipates and destroys all electricity, as for instance, when charged plates of glass or resin are presented to it; and that in other cases it collects electricity, as when it is applied to the summit of electrometrical points intended for meteorological observations. I thought, therefore, that something analogous took place here: but the dispersive effect being much superior, the positive pole was constantly discharged by drawing off the excess of the elective fluid, while by this very act the negative side was left

at

But this hypothesis will not apply here.

at a maximum of intensity. But a more accurate analysis of the phenomena, and a farther investigation of the facts that occurred, convinced me of the erroneousness of this hypothesis: for if it were by a simple dissipation of the electric fluid, that flame destroys the intensity at the positive pole, and carries it to a maximum at the negative, it must be perfectly indifferent, whether the flame were insulated or not.

Solids produce similar phenomena:

Now we have seen, that this is not the case. Besides, it will appear, that solid substances produce analogous phenomena, though inversely: so that here we have no expansible fluid to dissipate or accumulate the electric.

and flames from different substances differ in their action.

But what demonstratively proves the falsity of the hypothesis is the total difference, that exists in the mode of action of different flames, according to the chemical constitution of the bodies from which they emanate.

Flame not a distinct substance always of one nature.

It is a singular abuse of the abstract signs of language, to speak of flame as one constant homogeneous substance, whatever be the the nature of the matter undergoing ignition. This errour might have been pardonable previous to

Electricity and galvanism differ in their mode of operating.

the discoveries of pneumatic chemistry, particularly with respect to common electricity, the chemical effects of which are nothing, or difficult to ascertain. In galvanism, on the contrary, the chemical effects stand foremost: every physical effect is preceded or accompanied by chemical action; and it is precisely from this, that the discovery of Volta will for ever remain a memorable epoch in the annals of science. His pile is a landmark erected on the common frontier of chemistry and natural philosophy. A comparison of the mode of action of the flame of different combustibles soon decided the fate of my hypothesis.

Flame from hidrogen and carbon produces the preceding effects.

All flames arising from the incandescence of substances containing hidrogen and carbon produce the phenomena of insulating the negative pole, and acting as conductors to the positive, in the same degree.

Flame of sulphur a complete nonconductor.

Those, on the contrary, that contain neither hidrogen nor carbon, either do not produce this effect; as sulphur, the flame of which equally insulates both poles: or produce an effect totally opposite; as

Flame of phosphorus insulates the positive pole only,

phosphorus, which, in a state of ignition, insulates the positive, and conducts the negative. I shall enter a little into the detail of these facts.

On

On uniting the wires of the two poles with the flame of a wax or tallow candle, an oil lamp, yellow amber, camphor, volatile oils, and several other hidrocarburetted substances, the effects I have described, taking for example the flame of alcohol, will be observed fully. I had a strong reason, however, for proposing the latter, since the combustion of the substances here mentioned is scarcely commenced under the influence of the galvanic poles, before a fuliginous deposition takes place on each of the wires, particularly on that of the negative pole. This deposition is distinguished by a kind of dendritic vegetation, very striking on the negative wire, but much less distinct, and sometimes not to be perceived on the positive. These ramifications increase and spread with great rapidity, particularly at the negative pole: they tend toward one another from the negative to the positive, and the moment when these fuliginous filaments fill the space between the two wires, all electroscopic effect ceases, the circuit being closed by the conducting power of the carbon. The flame of spirit of wine, or of naphtha, is free from this inconvenience. If the experimenter would observe these fuliginous vegetations in the greatest energy, he should burn in a small capsule, oil of turpentine rectified by distillation. On bringing into this flame the two wires of a galvanic pile of tolerable strength, the fuliginous vegetations will be produced in such abundance, that frequently they will be seen to rise from the edge of the capsule, and form by their ramifications a very pleasing crown, the incandescent points of the tufts having a very rapid movement of tension on the fuliginous pedicle that supports them.

Flame of various substances.

Soon produced an arborescent soot,

especially on the negative wire,

that stretched from one to the other.

Alcohol and naphtha have not this effect.

Oil of turpentine has eminently,

and forms a pleasing exhibition.

To obtain the partial insulation of the negative effect, it is not necessary, that the substance from which the flame emanates should contain carbon. I filled my gazometer with very pure hidrogen gas, carefully washed; received the flame of a stream of this gas perfectly insulated between the two wires of a pile; and observed, that the effects during the whole course of the experiment were perfectly identical with those which I have described above with the flame of alcohol.

Carbon not necessary.

Hidrogen alone sufficient.

As to the flames emanating from substances that contain neither carbon nor hidrogen, it is very probable, that none of

Of other substances only sulphur and

phosphorus tried.

them produce the phenomena of the partial insulation of the negative effect. With respect to sulphur and phosphorus, I have proved this by experiment; and I am disposed to extend it by analogy to all substances of the same kind.

Flame of sulphur.

The uninsulated flame of pure sulphur, applied to either pole of the pile, acts as a perfect nonconductor. It is impossible to discharge either of the poles by the application of this flame; and the opposite pole shows no increase of intensity by this contact. Hence it follows, that the two wires connected by the same flame of burning sulphur remain equally insulated; and if a communication be established between this flame and the ground, it is still the same. The flame of sulphur, therefore, insulates the galvanic electricity as perfectly as the substance from which it emanates; and consequently the fourth class of effects do not depend on the dispersive property of flame, as flame.

The action does not belong to the flame itself.

Its connexion with chemical affinity shown by additions to the sulphur.

On the contrary, the intimate connexion of these phenomena with chemical affinities is demonstrated, by joining with the sulphur some hidrocarburetted substance. Thus on connecting the polar wires by the uninsulated flame of a match, or of a thread dipped in sulphur, the divergence is null at the positive side, and extreme at the negative.

Flame of phosphorus a conductor of the fifth class.

As to the flame of phosphorus, it exhibits a very remarkable property, in belonging decisively to the fifth class: that is to say, applied individually to each pole, it acts as a perfect conductor; but the moment the two wires are united in it, the positive side is found to be completely insulated, while, with respect to the negative pole, the conducting power continues in full energy. I shall not enter into the particulars of the experiments, as they were conducted precisely in the same manner as those already mentioned: but I shall observe, that, to satisfy myself whether the moisture adhering to the sticks of phosphorus, taken from under water, had any influence on the phenomenon, I several times took the precaution, carefully to wipe the pieces I intended to employ, and then to keep them a whole day in a phial filled with calcined muriat of lime. This perfect desiccation did not affect the phenomena. Neither did brown and opake phosphorus, obtained directly from distilling the acid with charcoal, differ in its effects, or in their degree, from

The experiment not influenced by the phosphorus being wet,

or impure.

that

that which I had brought by subsequent operations to that colour, semitransparency, and fracture, which indicate its greatest purity.

Perhaps we may infer from this, that the impurity of brown phosphorus does not arise, as some have supposed, from a portion of carbon carried over by the phosphoric vapours. In fact, if the smallest portion of carbon, burned with sulphur, immediately communicate to its flame properties absolutely different from those of the flame of pure sulphur, analogy leads us to expect similar effects from carbon incorporated with phosphorus. But I found nothing of this in the combustion of brown phosphorus. I am free to confess, however, that this is an argument of no great weight, particularly as the most essential point of comparison is still wanting, for I have never been able to succeed in burning together phosphorus and charcoal mixed in different proportions.

Brown phosphorus perhaps not rendered impure by carbon.

(To be concluded in our next.)

II.

Facts toward a History of Prussiates. *By Mr.* PROUST.

(Concluded from p. 109.)

Some Precipitations by the Simple Prussiate.

THIS prussiate, with the metallic solutions, gives different results from the triple prussiate, some of which had already been noticed by Scheele. The following are those I have observed.

Precipitates with the simple and triple prussiates differ.

Silver, with the triple prussiate, gives a white precipitate, which soon turns blue, in consequence of the white prussiate of iron mixed with that of the silver.

Silver.

With the simple prussiate it produces a white curd, that does not change.

Gold is not affected by the triple prussiate.

Gold.

With the simple prussiate it gives a white precipitate, that turns to a fine yellow. This precipitate is a true prussiate of gold, and does not fulminate by exposure to heat. Distilled in a retort it gives out water, empyreumatic oil pretty abundantly, and gaseous oxide of carbon that burns with a blue flame. The residuum is gold mixed with powdered charcoal. I find no mention of ammonia in my notes, whether it were forgotten I do not know.

Molybdic acid. Molybdic acid has no effect on either of the prussiates.

Tungstic oxide. Neither has oxide of tungsten.

Titanium. Titanium, with the triple prussiate, afforded prussian blue, in consequence of the iron retained by the oxide.

With the simple prussiate it gave yellow oxide of iron, such as this prussiate produces with solutions of red oxide. I have never yet been able to obtain titanium perfectly free from iron.

Uranium. Uranium gave a blood red precipitate with the triple prussiate. With the simple, a yellow white.

Cobalt. Cobalt gave a grass green precipitate with the triple prussiate. With the simple, a light cinnamon.

Nickel. Nickel gave a greenish white precipitate with the triple prussiate. With the simple, a yellowish white.

Manganese. Manganese gave a peach blossom precipitate with the triple prussiate. With the simple, a dirty yellow.

Copper. Copper gave a fine crimson with the triple prussiate. With the simple, a yellow.

Muriate of copper. White muriate of copper, or that in which the oxide is at a minimum, dissolved in muriatic acid, gives with the triple prussiate a white precipitate, but tinged with a little crimson. It appears, that the precipitate would be white, if the muriate were completely free from oxide at a maximum: but the solution of this muriate is like that of iron, it is difficult to keep it at a minimum of oxidation, in consequence of the action of the air.

With the simple prussiate this muriate gave a perfectly white curdy precipitate. A few drops of solution of potash took from it its prussic acid, and turned it yellow, which is the colour of oxide of copper at a minimum.

Platina. Platina afforded nothing with either of the prussiates: but I find a memorandum, to examine it again.

Prussiate

Prussiate of mercury is obtained, as is well known, by treating red oxide of mercury with prussian blue. This salt easily crystallizes in tetraedral prisms. It is always opake. It may retain potash, as will be seen presently, if there were any in the prussian blue. It equally retains oxide of iron, as may be seen by the following experiment. Heat a few grains with muriatic acid in a little matrass, and white prussiate will be precipitated. Prussiate of mercury.

To free it from iron, its solution must be boiled with red oxide of mercury repeatedly: each time it deposits oxide of iron, but this depuration is tedious. The prussiate of mercury changes its state by being boiled with red oxide, and appears to take up a surcharge; for it no longer crystallizes in prisms, but in small groupes of very fine needles. Their solutions too require to be farther concentrated; and dissolving the crystals afresh does not bring them back to their original figure. Freed from iron.

This salt heated in a retort is easily and wholly decomposed, if the fire be not urged too strongly. It is sufficient to heat a few grains in a tube of three or four lines diameter, closed at one end. If, while thus heated, the open end be exposed to flame, the prussic gas mingled with gaseous oxide of carbon takes fire. The flame is red and blue, terminated by a yellowish aureola. One hundred grains of prismatic prussiate gave one time seventy-two grains of mercury, at another seventy-two and half. The residuum of eight or nine grains was a mixture of charcoal and carbonate of potash. This is nothing extraordinary, for the alkali cannot decompose prussiate of mercury, and no doubt it was contained in the prussian blue, which was that of the shops. Decomposed by heat.

The products that arose in this distillation were ammonia; oil, and this even in tolerable abundance; and a mixture of carbonic acid gas, and carbonic oxide. Products.

There does not appear to be any prussiate of mercury with oxide at a minimum for its base; for the prussic acid applied to mild muriate of mercury, or to the nitrate with base at a minimum, eliminates a portion of the mercury, and produces a prussiate with base of red oxide, like that obtained directly by treating red oxide with the acid. The oxide of mercury always at a maximum.

Red oxide of mercury decomposes both prussiates of potash.

The red oxide of mercury equally decomposes the simple prussiate. The potash too is separated from it; and as this has no action on the prussiate of mercury, the prussiate crystallizes amidst it. It likewise decomposes the triple prussiate completely, but this requires long boilings. In this process, the black oxide in the triple prussiate passes to the state of red oxide, and is deposited as an ochre. Part of the mercury gives out the oxigen requisite for this, and hence it is found in the metallic state among the ochre precipitated; but without this superoxigenation of the iron, which diminishes the affinity of this metal, the oxide of mercury probably would not decompose a combination so solid as that of the triple prussiate.

Prussiate of mercury not decomposed by sulphuric acid diluted,

The aqueous sulphuric acid has no action on prussiate of mercury, even with heat. Not the slightest smell of prussic gas is given out.

Potash saturates the sulphuric acid as a vehicle of the prussiate, but occasions no precipitate.

Concentrated sulphuric acid destroys the prussic, gives out sulphurous acid, and thus destroys all means of comparison.

or by nitric;

Nitric acid is not more successful even with boiling. At the beginning, indeed, a little nitrous gas is perceived; but this, no doubt, is occasioned by the black oxide of iron contained in the prismatic prussiate. The prussiate, however, crystallizes in the midst of the acid; and alkalis saturate this acid without precipitating any thing.

but it is by the muriatic.

But it does not elude the action of the muriatic acid in like manner. There is a separation of prussic gas, a complete decomposition, and the prussiate is totally changed into corrosive sublimate. Accordingly, alcohol dissolves the saline residuum of this process completely; and we find nothing but sublimate on trying it by reagents. It is well known, that alcohol does not dissolve the prussiate of mercury.

Soluble in potash.

Potash dissolves the prussiate of mercury abundantly, by the assistance of heat; and this salt crystallizes in it on cooling. Alcohol separates it too, and it is found entire.

Decomposed by muriate of tin, and hidrosulphuretted water;

Muriate of tin at a minimum, and hidrosulphuretted water, decompose this prussiate instantly, and the prussic acid is set free.

It

It has been seen, that muriatic acid acts effectually on this prussiate: it might be supposed, therefore, that muriate of ammonia, which offers the prussic acid a principle capable of uniting with it, should make a change of bases; but it does not. If a solution of prussiate of mercury be heated with muriate of ammonia, nothing new is produced, and alcohol separates them completely. Potash and limewater precipitate nothing from the mixture, not an atom of corrosive sublimate; and the green sulphate of iron, which would not fail to form a prussiate of iron with that of ammonia, if it met with any in the liquor, does not experience the least change. but not by muriate of ammonia.

Prussic gas.

On heating 1440 grains of triple prussiate in a retort, with a sufficient quantity of dilute sulphuric acid, four ounces [2304 grs.] of alcohol were impregnated with about 80 grs. of gas. I kept the alcohol in a jar in a mercurial trough: the gas dissolved in it rapidly, but it would have taken up much more. The water of the intermediate receiver too was loaded with it: its smell was suffocatingly pungent, and its kernel flavour was extremely strong. This water did not render that of barytes turbid. The gas has a constant tendency to escape, and is perpetually raising up the cork. If a small matrass filled with the solution be immersed in hot water, the gas separates rapidly, and burns at the orifice: on bringing a candle near it, smoke is perceived; no doubt because a part of the charcoal escapes, as in the combustion of volatile oils. Prussic gas.

Prussic acid dissolved in water, and kept in a bottle closely stopped, is decomposed spontaneously. In four or five months it becomes yellow. It gradually loses its smell, grows turbid, and deposits a coffee coloured sediment, which, after being heated, exhibits all the characters of charcoal. Prussic acid decomposed spontaneously in water.

By distillation it affords a little water, prussic acid, and ammonia. The carbon is azotized; and it acquires one of the principles, which the acid relinquishes on being decomposed, for, on heating it with carbonate of potash, it afforded me a lixivium capable of making prussian blue. Decomposed by heat.

But

But while the carbon in separating retains azote, the greater part of the latter, combining with hidrogen, forms ammonia. Thus ammonia is found in the yellow liquor, with the remainder of the acid that has escaped decomposition.

Solution of prussic acid and sulphate of iron.

Prussic acid dissolved in water does not render the solution of green sulphate of iron turbid, till it has undergone the changes just mentioned; it then affords blue with it, from the concurrence of the ammonia recently formed.

Solution distilled.

Finally, this solution being distilled affords prussiate of ammonia, and nothing more is found in it but some particles of carbonaceous matter, which fall down. It would have been of importance to ascertain, whether there were any carbonic acid with the ammonia, but I forgot it at the time. I intend, however, to examine it again.

Solution of the acid in alcohol keeps very well. Inference.

The alcoholic solution keeps perfectly well. Hence we may even infer with some reason, that, as alcohol is better adapted than water both to dissolve and retain it, the prussic gas, considered too with respect to its qualities of being aromatic and inflammable, perceptibly approaches nearer to oily, combustible, and complex products, than to saline substances.

General deductions.

From these facts it follows, first, that there is but one prussiate of mercury, the base of which is at a maximum: secondly, that the augmentation of attractive power, which the prussic acid borrows from the black oxide, when it has to combine with potash, or the red oxide of iron, and on which Berthollet has so justly insisted, ceases to be necessary, when it has to unite with the oxides of gold, silver, copper, cobalt, nickel, uranium, mercury, &c. We see, in fact, with regard to the latter, this acid, the affinities of which are so sluggish, so little adapted to entitle it to the name, has notwithstanding no need of black oxide, to furnish with mercury a saline compound, very soluble, very crystallizable, and, in short, possessed of all the characters, that distinguish the most perfect compounds. To these anomalies let us add those it has of preferring mercury to all the alkalis; not yielding its oxide either to the nitric or sulphuric acid, each of which is so far beyond it in strength; and lastly, to yield it only to the muriatic acid,

which

which is in so many respects inferior to the sulphuric and nitric.

Lixivium of animal charcoal.

Equal parts of charcoal of blood and carbonate of potash, heated to redness in a covered crucible, always afforded me the richest lixivium. Prussic lixivium.

Supposing, that the carbonic acid might be an obstacle to the saturation of the potash, I added lime to the mixture, but the lixivium was not improved by it. Not improved by lime.

I heated red hot, for half an hour, a mixture of 144 grs. of charred blood, with as much carbonate of potash. After lixiviating, 104 grs. were left, 40 having been destroyed.

These 104 grs. were again treated with 144 of carbonate of potash, and were reduced to 62, so that the loss was 42.

The lixivium of each of these was saturated with the solution of the sulphate of iron of the shops; and the blue produced by the former, after brightening, was double that afforded by the second. First lixivium strongest.

To ascertain the influence of temperature, I tried three mixtures of equal parts. The first was kept red hot half an hour, the second an hour, the third an hour and a quarter. The lixivium of the first produced but little blue; those of the other two a great deal, and nearly in equal quantities. These results prove, either that the simple prussiate, which predominates in the lixivia, is preserved amid the carbonaceous alkaline mass, or that it is reproduced as fast as it is destroyed. The red heat must be kept up some time.

Powdered charcoal of blood grows moist in the air. By washing, it affords muriate of soda, and carbonate of soda united with a little prussic acid. Calcined blood deliquescent.

Charcoal of blood lixiviated with potash a second time still affords blue, though but little; a third time, the blue is less perceptible; a fourth time, there is none. This charcoal, thus exhausted, and heated red hot, incinerates with great facility, and without exhaling any smell of ammonia, as that does which is burned immediately after having been exposed to distillation. It seems as if it became more combustible in proportion as it parts with its azote, and approaches Exhausted by successive lixiviations. Incinerated.

proaches nearer to vegetable charcoal: nitric acid, however, cannot inflame it. As azote is capable of forming solid combinations able to resist a high temperature, what would be the influence of animal charcoal in the formation of steel? Workmen employ sheep's hoofs for casehardening; has their charcoal any advantage over that of wood?

Would it make superior steel?

Prussiate of ammonia afforded by it.

Equal parts of washed charcoal of blood and potash decarbonated by lime, or lapis infernalis, afforded me by distillation simple prussiate of ammonia, and a great deal of gas, which had the prussic smell, and burnt with a red flame.

Equal parts of the same charcoal and oxide of manganese afforded me carbonate and prussiate of ammonia.

But it does not answer for making sal ammoniac.

The desire of fabricating ammonia with advantage led me to the following experiment. I distilled a mixture of six drachms of charcoal of blood, two drachms of clay, and two of muriate of soda; but the product of sal ammoniac was less than I had expected.

Prussiates from some vegetable charcoals.

All vegetable charcoals azotized are fit for making prussian blue. Thus those of gluten, chich pease, indigo, and pitcoal, afforded me tinging lixivia, sometimes mingled with hidrosulphuret: those of sugar and sugar of milk did not give the slightest indication of blue.

Charcoals of chesnut tree and heath do not contain nitrogen.

The charcoals of the chesnut tree and heath, which smiths prefer, because they have the property of not burning any longer than they are blown, do not derive this from azote, for their lixivia contain no prussic acid.

Cream of tartar and sal ammoniac in certain proportions afford a prussiate.

Cream of tartar heated red hot affords a lixivium, which does not afford the least indication of it: two parts of cream of tartar and one of sal ammoniac, the same: but one part of sal ammoniac, with four of cream of tartar, yields a lixivium, that contains simple prussiate, and affords blue with the green sulphate of iron of the shops. Cream of tartar and nitrate of soda afford nothing.

This proves, that animal charcoal is preferable to vegetable on account of the azote merely. It also follows, that, if we should some time or other discover an azotized compound more capable of sustaining a strong heat than the ammoniacal salts, we might be able to form prussic acid, perhaps, in a less laborious manner than by means of animal charcoal.

Examination

Examination of the li ivia.

By distillation these constantly give prussic acid and ammonia, the origin of which we have seen above. Lixivia examined.

They also contain carbonate of potash in la ge quantity; simple prussiate of potash; triple prussiate of potash; sulphate of potash; phosphate of lime; and sulphur.

They let fall the phosphate of lime as they are evaporated; how it was sustained in them I know not.

If a portion of the lixivium be saturated with sulphate of iron, and the liquor with which the blue produced is brightened be examined, phosphate of iron will be discovered in it. It was this phosphate, that led Westrumb to suppose the acid of prussian blue to be the phosphoric.

Alcohol applied to the concentrated lixivia takes from them some simple prussiate; but it appeared to me difficult, to exhaust them of it by its means. The triple prussiate remains in the lixivium with the carbonate.

Of these two prussiates one only can produce prussian blue with the red oxide of iron, which is the triple prussiate, and this because it contains black oxide of iron. The simple cannot, because it is destitute of this black oxide: but it acquires this property, and is converted into triple prussiate, as soon as the lixivium is mingled with the sulphate of iron of the shops; and consequently, if a sulphate completely red be employed, we shall have much less prussian blue, because, the black oxide failing, it cannot form a triple prussiate and afford blue with the same sulphate. Two experiments will render this evident. Common sulphate of iron more advantageous for making prussian blue than the red sulphate.

I divided a lixivium into two equal parts. One was precipitated with red sulphate, the other with the green sulphate of the shops. The surplus oxides being separated by the brightening liquor, the blue from the second was found to be to that from the first in bulk as four to one. Experiment.

The first lixivium, when filtered, had a strong kernel smell. I saturated it with potash, to fix the free prussic acid afresh; and on trying it afterward with the red sulphate, it did not afford an atom of blue, but with the green it yielded a great deal. Hence we may conclude, that a carbonaceous lixivium cannot yield all the blue it is capable of producing with the

solution

solution of red oxide, without the assistance of black oxide. Hence the risk of losing all the simple prussiate contained in a lixivium, if we were to use only a sulphate, the oxide of which is completely red; though this I formerly recommended, but it was from mistake. I was not aware, that, if the green sulphate have the inconvenience of affording a pale prussian blue, the oxigen of the atmosphere soon remedies this; and that it has the essential advantage of furnishing the simple prussiate with that portion of black oxide, which is necessary to convert it into a triple salt, and enable it afterward to produce blue with the red oxide. Thus practice had attained the object before theory: but practice in turn becomes a rational process, as soon as theory comes to its justification. Two other experiments will corroborate this.

The blue from the green sulphate is pale at first, but grows deeper by exposure to the air.

Alum employed,

The lixiviums are commonly precipitated by a solution of four parts of alum, and one of the sulphate of iron of the shops.

but it has no chemical effect in the process.

I divided one of these solutions into two parts. One was superoxided by the oxigenized muriatic acid, the other was not. I afterward saturated them with the carbonaceous residuum. The common solution afforded abundance of blue, but the superoxided yielded only a pale precipitate, which was nothing but a little blue diffused among a great deal of alumine. This experiment does not differ at bottom from the preceding: it has only the advantage of showing, that alum is merely a passive ingredient in forming prussian blue.

The lixivium from blood not precisely the same as that from prusssan blue itself.

The lixiviums of the manufacturer, therefore, are not like those made by treating prussian blue with an alkali. The latter will always afford abundance of blue, because it is made a triple salt in the operation itself: but this is not the case with the former; they afford blue only in proportion to the quantity of triple-salt they contain, and to augment this, or to convert their simple prussiate into it, it is indispensable to employ a sulphate, that, if not strictly green, at least is so in a certain degree; and this is precisely the case with the sulphate of the shops, however long it has been made.

In the calcination of the blood probably

From these details we learn farther, that, if the lixiviums contain but a certain portion of triple prussiate, it is either because

because the charcoal of blood has not iron sufficient to convert all the simple prussiate that is formed during the calcination into a triple salt, or that part of this salt is again reduced to the state of simple prussiate by the loss of part of its oxide, as we have seen takes place when it is heated alone. Of these two opinions, however, I should be inclined to adopt the latter, because I have observed, that the ashes of the charcoal that has been lixiviated always afford a great deal of iron: we have no reason, therefore, to suppose, that in calcining the carbonaceous alkaline mixture iron is wanting to the prussiate; and indeed, if we reflect on the subject, it is surprising, that the triple prussiate, which actually exists in the lixiviums, should have been capable of defending its oxide against the effects of the carbon continually tending to reduce it. The whole of this, however, is at present very obscure: we neither know the period when the prussic acid is formed, whether it be destroyed to be again reproduced, nor, lastly, the degree of heat required to obtain the greatest possible quantity of either of the prussiates, that are the objects of the manufacturer.

part of the triple prussiate destroyed.

The existence of the triple prussiate in the lixivia may be demonstrated by the following experiment.

Proof that the lixivia contain triple prussiate

Saturate a lixivium with aqueous sulphuric acid. The carbonic acid first flies off, and next the prussic acid of the free prussiate. After this, heat must be applied, when the triple prussiate is attacked, and the white prussiate of iron is made apparent. Beside this, concentrated lixivia long kept deposit octaedral crystals of triple prussiate.

The prussic lixivium has two very distinct tastes; one of potash, the other of kernels. By the latter we may judge at once of its quality. If it affect the palate but slightly, the lixivium is defective, either because the mixture was insufficiently heated, or the animal charcoal in too sparing proportion. I am of opinion too, that the calcination of the mixture in the open air does not contribute to the augmentation of the prussiate; and that it would probably be more advantageous, and save trouble, to heat it in covered crucibles in a reverberatory furnace, since it has been proved, that stirring the mixture is not necessary to the success of the operation.

Their quality may be judged by the kernel taste.

When

Previous to evaporation a little green sulphat should be added, to prevent the loss of the simple prussiate.

When it is necessary to concentrate the lixivium, either to keep it, or that it may take up less room, we should previously take care, as Curandeau perceived, to prevent the simple prussiate from being destroyed. This is readily done by adding green sulphate in small quantities. In this way it dissolves completely in the lixivium, which first grows red, then becomes again yellow. An excess of sulphate does not alter it, because the potash, which predominates, reduces it to an oxide; and this falls down, without being able to pass to the state of a prussiate. To attain this, it must present itself accompanied by an acid; for the oxide here spoken of is entirely that at a minimum, which has no action on the triple prussiate.

Advantage of this.

I have divided a lixivium into two equal parts: one was prepared, or converted into a triple salt, by the green sulphate; the other was not. I afterward distilled them: and the first gave no indication of ammonia; the second furnished it as usual. It is indispensably necessary therefore, to prepare the lixivia before they are concentrated. Finally,

The red oxide does not unite with the simple prussiate,

neither the red oxide, nor its sulphate, as Scheele found, is capable of dissolving in the simple prussiate, and giving it the properties of the triple. This oxide too, though adapted to become the basis of prussian blue, is equally unable of itself,

or decompose the triple prussiate alone.

to decompose the triple prussiate; it must be presented to it in solution by an acid.

Recapitulation.

Component parts of prussic acid.

Prussic acid is composed of carbon, nitrogen, and hidrogen, in proportions of which we are yet ignorant. We can only conjecture, from the great quantity of carbon it leaves in several instances after it is destroyed, that this principle enters into its basis in a very large proportion, compared with the others.

No oxigen.

Neither is there any fact, that indicates oxigen to make a part of it; and indeed, from the well known affinities of its three elements, added to the circumstances under which the acid is formed, we can scarcely think it does.

Has few acid qualities.

The prussic acid in its separate state has very few of the common qualities of acids. It has not a sour taste; it does not

not redden litmus; it does not dissolve in water, the proper menstruum of acids, so well as in alcohol; and it is even decomposed in it spontaneously without access of air. With alkalis it forms combinations so imperfect, that we find in them the specific properties of their component parts scarcely altered; and the weakest of all acids, the carbonic, is capable of decomposing them. In short, its combustibility, taste, smell, generation amid volatile oils and kernels, and quality of keeping in alcohol, form an assemblage of properties, by which it approaches much nearer to oily and inflammable productions than to saline substances.

More analogous to oils and inflammables.

The prussic acid, however, notwithstanding its little saline energy, attacks the oxide of mercury at a maximum with great advantage; and with this oxide it furnishes a saline combination so strongly characterized in its qualities, that we are obliged to acknowledge it acts under certain circumstances as one of the most powerful of acids. In fact, the prussiate of mercury wants nothing to entitle it to rank with the most perfect metallic salts; and what may perhaps be deemed astonishing is, to see it refuse to combine with the oxide at a minimum; yet, from the effect of concurrent affinities, of which there are other examples, it raises it to the state of an oxide at a maximum, separating one portion of the metal, to form a prussiate with the other.

Yet toward oxide of mercury it acts as a powerful acid.

Pussiate of mercury.

Prussic acid has no action on the red oxide of iron; but it very readily attacks the black oxide, and produces with it white prussiate. It is true this prussiate is not strictly white, on account of the difficulty of preparing a precipitate with the green sulphate totally free from a surplus of oxide; and accordingly it is always greenish: but as it becomes a perfect prussian blue by drying, there can be no doubt, that the prussic acid and the base of green sulphate, if not affected by contingent circumstances, would furnish a prussiate as white as that which we obtain in a more easy manner.

Action of prussic acid on iron.

Prussian blue is not a single compound, as had been supposed. This the following observation is sufficient to show. It is known, that the base of this blue is the red oxide of iron: but, if this oxide alone were sufficient to make prussian blue, why is not this blue afforded by prussic acid and red oxide? and why do not the alkaline prussiates produce it with

Prusisan blue not a single compound.

with solutions of this oxide? Something else therefore is wanting to the prussian blue, and the following facts will complete the proof.

Proofs of this.

On applying potash to prussian blue we obtain a yellow chrystallizable salt, which has always a constant proportion of black oxide of iron. If we employ this yellow prussiate to reproduce prussian blue, the oxide repasses into the new combination with the prussic acid. The black oxide then is a necessary element in the formation both of the crystallizable prussiate and of prussian blue; as well as of all the metallic prussiates, that are prepared with the triple prussiate of potash.

Prussiates formed by different metals.

There are some metals, that are capable of forming both simple and triple prussiates, as copper, silver, manganese, cobalt, nickel, uranium, &c. There are others, that form only a simple prussiate, as gold, mercury, &c. There are some, that admit only of a triple prussiate, as iron, &c. And lastly others appear to be incapable of combining with the prussic acid. Except prussian blue, however, and the prussiate of mercury, we know but little of them, and they deserve farther examination.

Union of black oxide of iron with the prussic acid.

The black oxide of iron combined with prussic acid can pass from one combination to another without changing its state. The base of this combination may even be raised from a minimum to a maximum, without the black oxide participating in the change. The combination of the acid with this oxide is bound by an affinity so powerful, that the alkaline hidrosulphurets cannot separate them; or attack the oxide, if you please, either in the triple prussiate, or in the prussian blue.

The prussic acid, combined with that portion of black oxide which enables it to form triple prussiates, either alkaline or metallic, is a peculiar combination, the existence of which is not doubtful, but of which we know nothing separately from these prussiates.

Heat reduces the triple prussiate of potash to simple,

The triple prussiate of potash cannot support a red heat, without losing the black oxide, and consequently being reduced to a simple prussiate.

and decomposes the simple prussiate.

The simple prussiate is decomposed likewise, but by a far lower temperature: its acid is destroyed, and reduced to ammonia and carbonic acid: and it is the destruction of this

salt

salt by the heat of ebullition, that injures the lixivia for preparing prussian blue.

The simple prussiate assumes the character of triple prussiate, as soon as black oxide of iron, or a salt with this oxide for its basis, is presented to it; and thus acquires, beside the advantage of being crystallizable, that of not being decomposable at a boiling heat. Simple prussiate converted into triple.

This prussiate, which was the test liquor so much sought after by chemists, does not afford prussian blue with solutions of red oxide of iron; but it produces this blue if they contain any black oxide, because its acid immediately attaches itself to that portion of the black oxide, which will serve as an intermedium between it and the red oxide. The testliquor.

The triple prussiate of iron, or prussian blue, strongly heated, is reduced to ammonia, carbonic acid gas, gaseous oxide of carbon, steeled iron, and carbon. Decomposition of the triple prussiate of iron,

The prussiate of mercury affords the same products by its decomposition, and likewise a certain portion of oil. & of mercury.

The lixivia of the carbonized materials contain little triple prussiate, but a great deal of the simple; and they must not be boiled down, till the constitution of the second is strengthened by an addition of black oxide, or of green sulphate. The lixivia.

To obtain from these lixivia all the prussian blue they are capable of affording, it is indispensably requisite, to employ a sulphate of which a portion at least is green; without which the simple prussiate they contain cannot furnish blue with a sulphate of which the base is completely red. How to obtain the most blue from them.

To conclude, if the reader take the trouble to compare this paper with Scheele, he will find, that all the truths it contains were perfectly known to him; but I conceived they required to be more fully explained, which I have here attempted.

III.

An Account of a Stove for Heating Rooms, or Drying different Articles; by Mr. G. Field, *of Newman-Street.**

THE various advantages of heating, boiling, steaming, evaporating, drying, ventilating, &c., are united in this stove; so

* Transactions of the Society of Arts, for 1806.

so that it is capable of being applied to many useful purposes, both in domestic economy and the arts: on which account, a silver medal was voted by the Society of Arts to the inventor. The subjoined description, with the annexed plate, will more fully explain its design.

Description of the stove.

Fig. 1. Pl. VII. Represents a longitudinal section of the stove, showing the course of the air from its entrance into the flues of the stove at A, to its entrance into the upper chamber of the stove at B: and also, the course of the smoke from the fire-place at C, till it escapes from the stove at D. E, E, are the doors or openings of the fire-place and ash-hole.

Fig. 2. Is a similar section at right angles with the above, exhibiting the course of the air through the chambers of the stove, from its entrance into the chamber No. 1. at B to its entrance beneath the fire-place at F. This figure also shows sections of the flues, with the divisions through which the air and smoke pass separately, the smoke flue in the centre, and the air flues on each side. G, G, are doors and openings through which the articles to be dried are introduced into the chambers.

When the fire is lighted, and the doors of the chambers, ash-hole, and fire-place, closed, the air by which the fire is supplied enters at A, Fig. 1, passes through the air-flues *a*, *a*, *a*, *a*, enters the upper chamber at B, traverses and descends through the chambers No. 1, 2, 3, and arrives beneath the fire at F, Fig. 2. Having supplied the fire with oxigen, it passes through the flue with the smoke, and escapes at D, heating in its protracted course the chambers and air-flues.

Its use as a drying stove,

As the cold air enters the stove at A, immediately above a plate forming the top of the fire-place, and pursues a similar route with the fire-flue, it enters the chambers very much heated and rarefied. Hence any moist substance placed in the chambers evaporates, in consequence not only of the heated flues circulating round them, but of a stream of warm rarefied air, which, while it continually causes evaporation, as continually bears away the exhaled moisture in its passage to the fire, thus imitating the gradual and efficacious plan of nature in drying by the sun and air. While these effects are taking place within the stove, part of the air which enters at A, Fig. 1 and 2, passes through air-flues on the other side

and in warming rooms.

of

Mr. G. Field's Stove for heating or drying.

Fig. 1. Side.

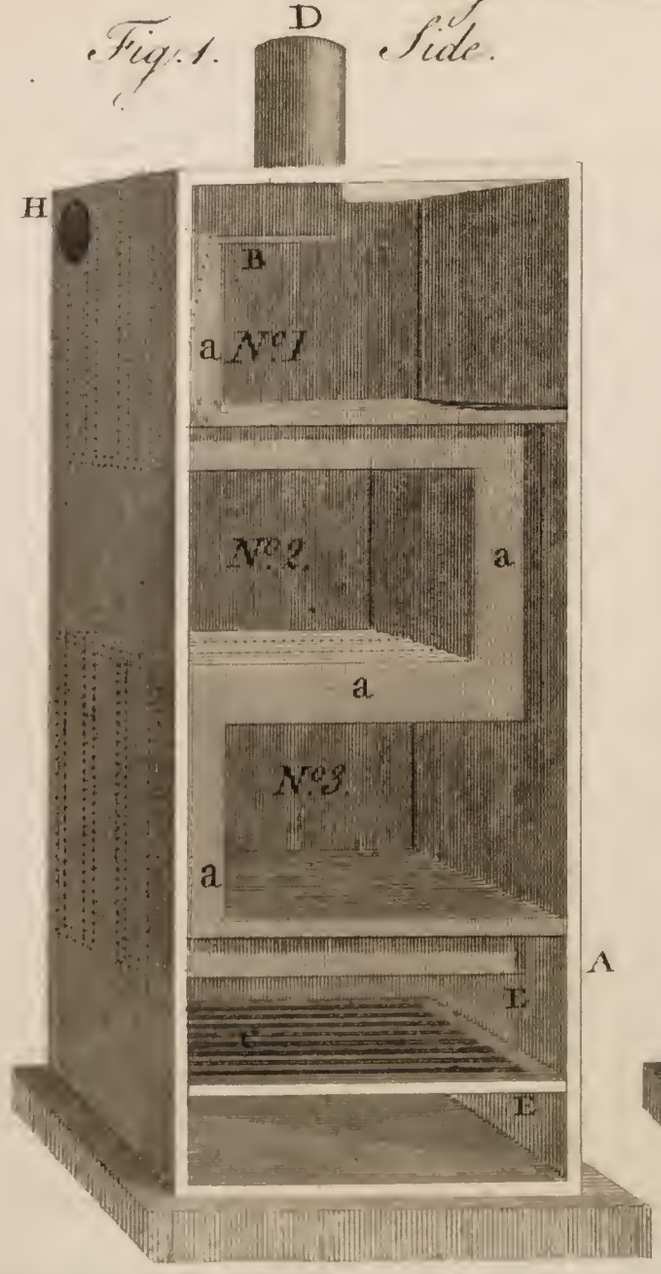


Fig. 2. Front.

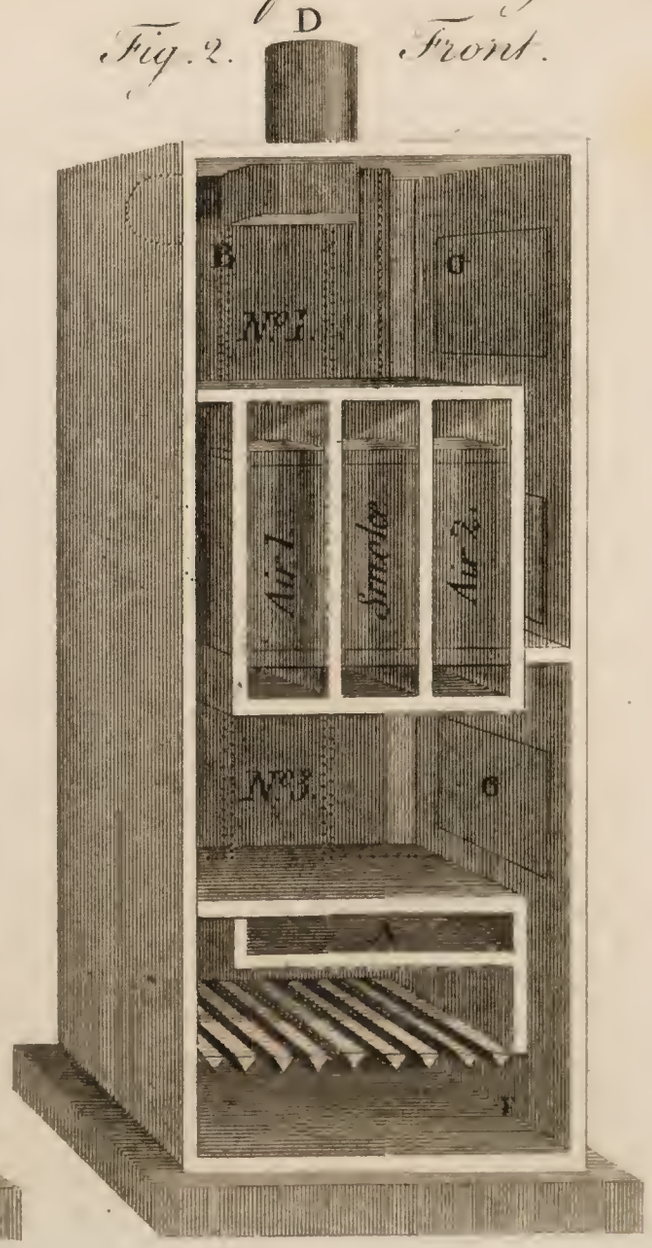
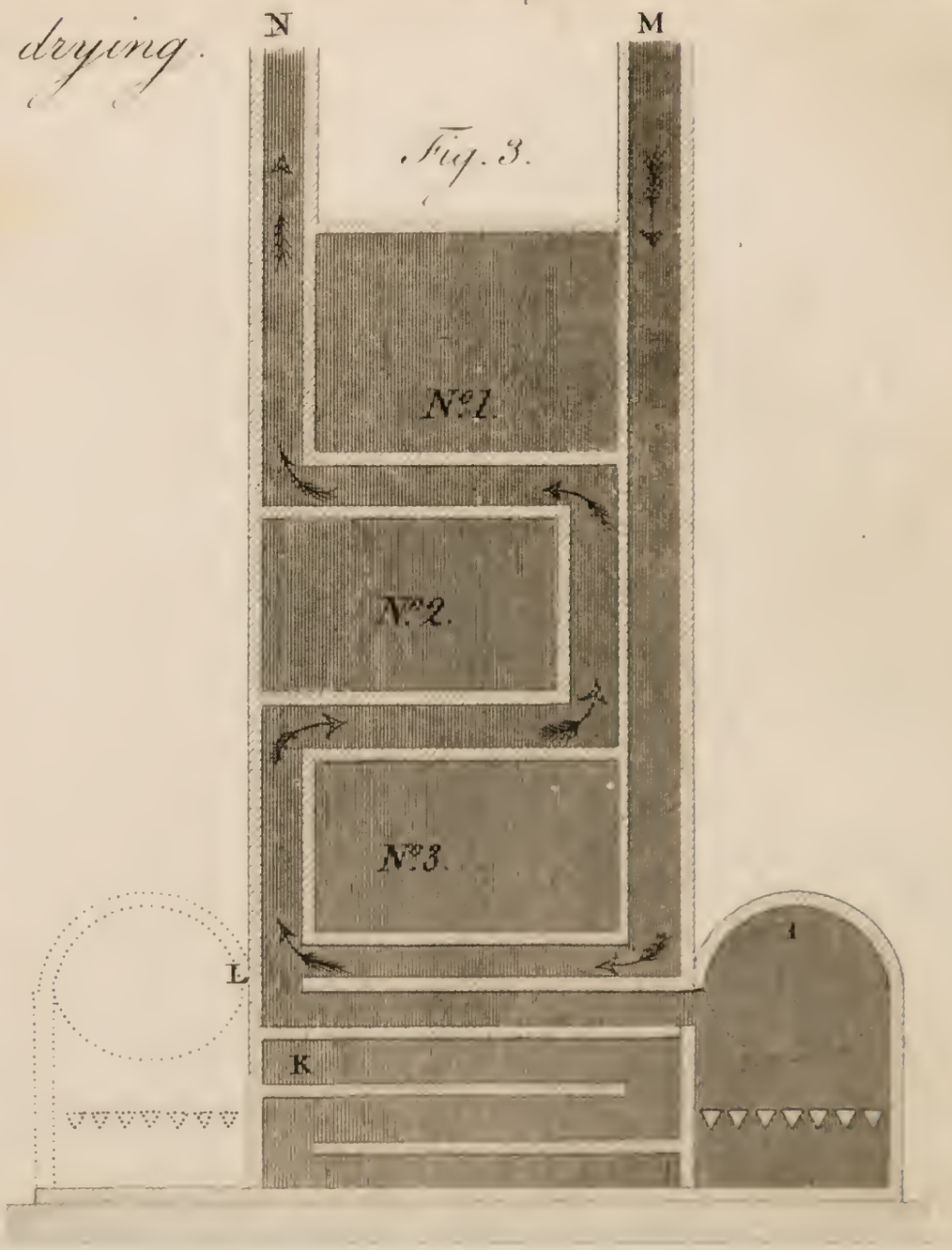


Fig. 3.



of the fire-flue, pursues a parallel course with the first, and gives out a current of warm air to the room at an aperture H. This effect may be obtained in a much higher degree, if the doors of the chambers and ash-hole are opened: should the hand or face be then brought near, they would be fanned with a stream of warm air, especially from the upper chamber.

Milk evaporated to dryness without burning, and other delicate processes.

By means of this stove I have evaporated milk to dryness, without burning or discolouring it; and have dried cherries, plums, and other fruits, so as to imitate those which are received from abroad. I have repeatedly dried colours and the most delicate substances without the slightest injury to them, even though the operation proceeded quickly.

Construction.

The height of the stove is about $5\frac{1}{2}$ feet; its diameter $2\frac{1}{2}$ feet, and that of the flues 4 inches. The external part is constructed of brick, and the internal parts of thin Ryegate or fire-stone, except the top of the fire-place, which is a plate of cast iron. Were it to be wholly formed of iron, its effects would necessarily be more powerful.

The plan may be extended.

Fig. 3. Represents an extension of the plan, in which stoves of this kind may be advantageously connected with one or more furnaces for chemical or other uses. The fire-place, brought out, either in front or on one side, by the present position of its crown I, forms a reverberatory furnace, or will make a sand-bath by reversing it.

The space occupied by the fire-place in Fig. 1 may in this be converted into apartments for evaporating substances, or occasionally for cooling them by an opening at K to admit cold air, while the warm air of the stove is excluded by a register or door. The dotted lines show the manner in which a second furnace may be connected by an opening into the flue at L.

Various purposes to which it may be applied.

In addition to the uses already pointed out, this stove would probably be found extremely serviceable in drying japanners' goods, and consuming the noxious fumes and gas which arise from the oil and varnish used in this business.

Since the stove is not limited to any certain dimensions, it might be adapted to the drying of malt and hops, perhaps of herbs, corn, and seeds generally. It might also be accommodated to the purposes of the sugar-bakers, connected

with the great fires they employ for their boilers. It has been shown to be useful in the confectioners art, and probably it may be equally so in baking biscuits for the navy; nor less so in drying linen for the laundress, dyer, calico-printer, and bleacher. I have myself found it well accommodated for a chemical elaboratory.

The efficacy of the stove in ventilating, boiling, and steaming may easily be shown. In manufactories and rooms generally the heated and noxious part of the atmosphere ascends towards the ceiling: if then the air-flue M, Fig. 3, is continued upward according to the height of the room in which it is placed, the air will be drawn from the top, and the room become ventilated, while from the opening at N it is supplied, if requisite, with warm air.

It is unnecessary to show the various ways in which a boiler may be connected with this plan: it is sufficient to observe, that in the space allotted for the fire-place in Fig. 1, there is sufficient room within the body of the stove for this purpose; and that if the circulating air be made to pass over the boiler, evaporation may be carried on very expeditiously by the air removing the vapour as it arises. Finally, if another division of the flues be made in the manner shown Fig, 2, it might form a steam-pipe or flue, running the course of the air and fire-flues, to convey steam to one or more apartments of the stove; or extended beyond the stove for heating the room in which it stands. One of the air-flues might occasionally be adapted to this use. It is obvious that the power of steam in a heated apartment would be not only greater, but better kept up. In steaming it would be necessary to close the apartments of the stove, and to give air to the fuel by a different course.

Its form and dimensions admit of considerable variations.

As the stove is not confined in its dimensions, so neither is it necessarily of the form described in the drawing, nor are the apartments necessarily three: all these particulars admit of variation according to the local or other circumstances. It is evident that the air-flues themselves may be converted into chambers for drying, &c.; and the fire-place of Fig. 3 is well adapted to receive an apparatus for the decomposition of coal, &c.; for producing all the effects of the thermo-lamp, or illuminated smoke, &c. But it is needless to enumerate

iterate the many economical and philosophical uses to which the stove may be applied. It is sufficient for the present purpose, if I have rendered the principle and plan intelligible, the artist and manufacturer will then be at no loss in adapting it to the particular object, which he may require to accomplish.

A certificate from Mr. S. Sellers, Chemist, Broad street, Bloomsbury, stated, that the effects of the stove in question are as Mr. Field has described them in his paper.

IV.

Inquiries concerning the Oxidations of Iron; by Mr. DARSO.

(Continued from page 226.)

Oxides by Solution.

THE solutions of iron afforded me still more satisfactory results, both because they confirm and render more clear those obtained by calcination, and because they may throw much light on a number of manufactories, and simplify the chemical theory of iron. But, convinced as I am of these results, I offer them only as conjectures, since they are diametrically opposite to the present mode in which solutions of iron are viewed, and I am always afraid of being led into errour. Solutions of iron.

The inconstancy of the green oxide, which Lavoisier and Mr. Proust obtained when 100 grains of iron had taken up 37 of oxigen; and which, from the experiments I have related, varies from a few hundredths to 32; could not but lead me to conclude, that the properties distinguishing this oxide from the red, are owing not to a fixed degree of oxigenation, but rather to a certain density, which allows the water, or the acid, or both, to lodge in the interstices of each molecule; and hence the difference of colours of these precipitates by alkalis, prussiates, and gallates, and their less or greater solubility; the only properties that distinguish the green salts from the red. Green oxide variable. Why?

White oxide.

This reasoning led me to examine not only the red and green oxides, but at the same time the white oxide lately announced by Mr. Thenard*, of which I had beforehand some doubts†. I am sorry to call in question the labours of men whom I greatly esteem and respect, but I conceive I am seconding their views, if my observations be just.

Of the White Oxide.

Its characters.

The characteristics assigned to this oxide by Mr. Thenard are, 1st. that it becomes green by exposure to the air; 2d. that it is changed to green or yellow by oxigenized muriatic acid; 3d. that when the precipitate is made in a phial, and care is taken to stop it close, an absorption is observed on shaking it to convert the white oxide to green; which proves, that part of the oxigen of the air in the phial combines with the white oxide, and changes its colour. I shall take the liberty of examining these facts.

Change of colour in precipitates no proof of a different dose of oxigen.

From the experiments of Messrs. Fourcroy, Proust, and Berthollet, Mr. Thenard, and all the chemists of the present day, are of opinion, that the change of colour in precipitates does not in general indicate a different degree of oxigenation. The white is that which very frequently conceals the true colour of oxides in almost all metals, that are said to be susceptible of oxigenation, as tin, mercury, copper, silver, lead, bismuth, and probably manganese. This depends on the quantity of acid the precipitates retain,

* See our Journal, Vol. XIV, p. 224.

† Before undertaking my present inquiry I could not avoid doubting, that a few hundredth parts of oxigen could modify the colour of metallic oxides so far as to change them from white to green, black, &c. All the facts with which I am acquainted militate against this property of oxigen; consequently, I was suspicious of every precipitate, the colour of which varied much from that of the oxides of the same metal obtained by calcination. If white, I argued, be the result of the combination of iron with a few hundredths of oxigen, why does not this colour present itself in the course of the calcination of iron? and why in the calcination of manganese, copper, and bismuth, do we not perceive the same phenomenon? Besides, the only well established white oxides are those of antimony, zin, and arsenic; and from the moment these become white they never change their colour, even from a considerable over dose of oxigen.

they

they being freed from this with more or less facility, according to their nature, and still more according to the circumstances under which the precipitation has taken place. This appears to be the case with the white precipitate of iron.

Conditions requisite to precipitate iron white.

The conditions requisite to throw down a white precipitate from a green salt of iron, are: first, that the solution be highly concentrated; second, that the precipitating alkali be in some degree the same. This did not escape the sagacity of Mr. Thenard, who recommends boiling dilute sulphuric acid on an excess of iron filings, that the formation of the white oxide may be more sure to succeed. In fact, whenever a tolerably concentrated solution of alkali is added to such a solution of iron, the alkali first seizes a portion of the acid, and probably a little of the water, and precipitates a white sulphate of iron, which frequently crystallizes, though irregularly, at the moment of precipitation, and still retains a sufficient quantity of acid, to be soluble in water. It likewise constantly turns sirup of violets green, and forms a red precipitate in water coloured with litmus, as is common to salts of iron with excess of oxide*.

Action on sirup of violets and litmus.

Proofs of the fact.

It is easy to verify this fact in a convincing manner, by letting fall a few drops of such a solution into an excess of alkali. If, after having left the white precipitate in it eight or ten minutes, the fluid be decanted, or if it be drawn off

* I have obtained several salts of iron, which immediately produce a red precipitate with infusion of litmus, and at the same time turn sirup of violets green. The property of precipitating infusion of litmus always indicates a salt that is at least neutral, and must not be confounded with the effect of simply changing its colour. In the mere change of colour the small portion of alkali in the infusion combines with an excess of acid, and quits the colouring matter, which it modified. There can be no doubt, that a salt exhibiting this phenomenon has an excess of acid, since there is sufficient to saturate the alkali in the infusion, without occasioning a precipitate; and on this principle is founded the use of this reagent. When the change of colour is accompanied with a precipitate, the alkali, not finding an excess of acid to combine with, seizes part of that which held the oxide in solution, and the precipitated oxide carries down part of the colouring matter with it. What appears extraordinary is, that oxide of iron does not turn sirup of violets green, though neutral salts of iron, or salts with an excess of oxide, have this effect.

Infusion of litmus gives a precipitate in certain cases.

Anomaly of oxide of iron.

by

by a siphon, which is the surer way; and a few drops of water be afterward added, to take up the alkali, that adheres to the surface of the precipitate and the sides of the glass; a mass of sulphate of iron will remain, great part of which will dissolve in water, and exhibit all the properties I have just mentioned. I have left this precipitate for twenty-four hours macerating in potash, and at the expiration of that time have still obtained soluble sulphate of iron.

Ammonia shows this more strikingly.

In a concentrated solution of ammonia, the result is still more striking; because, as the density of the precipitated sulphate of iron greatly exceeds that of the ammonia, the precipitate falls quite to the bottom of the glass, and great part of it sticking to this, escapes the subsequent action of the alkali; which does not happen with the concentrated fixed alkalis, the density of which is greater than that of the ammonia, and in consequence they envelope it on all sides.

Why a green precipitate is thrown down.

This is the reason why these solutions, which when concentrated throw down a white precipitate, throw down a green precipitate when they are diluted with water freed from air by long boiling. For this reason, limewater never gives a white precipitate, even with the most concentrated solutions. Lastly, for this reason I suspected before hand, that the muriates and nitrates, which give white precipitates with the alkalis, would be precipitated green by barytes and strontian water, which in fact is always the case. The green colour cannot be attributed to a superoxidation occasioned by the air in the lime, barytes, or strontian water; for, beside that the rapidity of the operation, and the quantity of air that so small a bulk of distilled water could contain, would not justify such an idea, I took the precaution to boil the water in which I dissolved the three earths for an hour and half.

Not from additional oxigen.

The same if diluted even with sulphuretted hidrogen.

If, instead of diluting the solution with water, it be diluted with sulphuretted hidrogen, in which the presence of oxigen cannot be suspected, the result will be still the same; the precipitates formed by alkalis will be green or black, and never white. It is true Mr. Thenard says, that, on pouring sulphuretted hidrogen on a red solution of iron, this will

yield

yield green or white precipitates. I am inclined to think, however, that this is rather an inference drawn by Mr. Thenard from his manner of considering the white oxide of iron, than a fact that came under his observation. Be this as it may, I can assert, that, having repeated these experiments several times, frequently changing the reagents, and varying the circumstances as much as possible, I have obtained only green or black precipitates, according to the concentration of the sulphuretted hidrogen, and the quantity of the sulphate of iron, presented to each other. If this assertion of Mr. Thenard were a fact he observed, I confess it is an anomaly, for which I cannot account, and which I cannot reconcile with the whole of the facts I have related.

The existence of a green oxide questionable.

If all these facts do not controvert the existence of the white oxide, I have still another to add, which not only controverts it, but even renders that of the green oxide questionable.

Let a phial be filled with three parts of ammonia, and one of sulphate of iron precipitating white; and let it be corked immediately. At first, a white precipitate will be formed, which on shaking the phial will dissolve in the ammonia. If the oxide of iron be afterward thrown down by means of water or of an acid, it will always be green or brown: and if, instead of precipitating it by either of these means, a small curved tube be fitted to the phial, its extremity be immersed in water, and sufficient heat be applied to expel the ammonia; as this is volatilized, a black or brownish oxide will fall down, which, on being dissolved in muriatic acid, will afford precipitates for the most part red. Now it is impossible here to suspect a superoxidation by the ammonia.

White salts of iron with base of red oxide.

Add to this, there are likewise salts of red oxide of iron with excess of oxide, that are not only white and soluble like the salt of Mr. Thenard, but frequently crystallize; that are not deliquescent, like the common red salt of iron; and that exhibit still other peculiarities. I shall mention these in a paper on another subject, where I shall speak of iron incidentally. Till that occasion, likewise, I shall defer explaining the cause, why some of the white precipitates observed by Mr. Thenard retain their colour even after long boiling.

boiling. The fact is very true, but oxigen has nothing to do with it.

Effects of oxigenized muriatic acid.

The changes occasioned, according to Mr. Thenard, by pouring oxigenized muriatic acid into a sulphate of iron precipitating white, agree very well with the idea I have formed of this precipitate. As oxigenized muriatic acid is so little soluble in water, and the sulphate of iron in question so concentrated, it follows, that, if the acid be not very abundant in the solution, the precipitate will be green; because the little oxigen contained in the muriatic acid is capable of converting to red oxide but a small portion of the green, which is so predominant as to envelope the red, and prevent its appearance. It will be the same to the eye as if the solution were diluted with water, equal in quantity to the muriatic acid, by which the white precipitate would have been changed to green in a similar manner. If, on the contrary, the oxigenized muriatic acid be very abundant, there is no doubt, that the solutions will become red, as is the case with all the green salts of iron.

On precipitating in a closed phial, air is not absorbed but evolved.

With respect to the third fact, namely, that on making the precipitate in a phial, and stopping it immediately, absorption takes place, and the residual air extinguishes a candle, I shall only say, that instead of absorption, I have always found an evolution of air, which has sometimes forced out the cork. It is true, that, after the white precipitate has passed to green or red, the residual air sometimes extinguishes a candle; but this is owing to the extrication of a principle, of which I shall speak presently.

Of the green oxide.

Green oxide offers three questions to be solved.

The green oxide obtained by dissolving iron in acids, offered me three leading facts to examine. First, to determine how much oxigen iron takes to pass to the state of green oxide: secondly, to account for its colour: thirdly, to observe the influence of atmospheric air on these solutions.

Proportion of oxigen.

To determine the proportion of oxigen, and observe at the same time the influence of the atmospheric air, I took 90 grains of iron filings, which I divided into three equal parts. Each of these was dissolved separately in muriatic acid diluted with

with water. As soon as the solution was complete, I precipitated one portion of 30 grains by ammonia, washed and drained it as quickly as possible, and dried it at a temperature of about 120° [300° F.]. After it was dry I found it a brown oxide, attracted by the magnet, weighing 36½ grains, and precipitated red from its solution in muriatic acid. Another portion I precipitated likewise by ammonia; but with a view to obtain the precipitate red, I diluted the solution with five or six parts of water at 50° [144°] before I added the ammonia. This oxide was in fact red; and when dried like the preceding, it was not at all affected by the magnet, though it weighed only 36 grains. Lastly, I precipitated the other 30 grains by ammonia likewise, using a very broad vessel, in which I left the precipitate exposed to the air for a month, stirring it twice a day. At the end of this time I dried it like the preceding: it was red, gave no signs of magnetism, and weighed 36·2. The only difference between all these oxides was, that the first was brown and magnetic, while the others were red, and did not become magnetic till exposed to a higher temperature.

Brown magnetic oxide.

Red, and not magnetic.

The same.

Yet all equally oxided.

Though I perceived in the course of this experiment, that it was not sufficient to establish with accuracy the proportion of oxigen, that the green oxide contains in solutions of iron by acids, on account of the oxigen that must combine with it during its being dried at so high a temperature, and in a state of such minute division, it confirms two of the principal results obtained in the oxides by calcination. This process afforded me red oxides that had only ·15 or ·20 of oxigen and the solution produced red oxides that contained only ·20 of oxigen, including what was absorbed during the drying. Calcination afforded red magnetic oxides; and solution did the same.

This does not ascertain the proportion of oxigen obsorbed during solution.

There are two methods of appreciating with extreme accuracy the quantity of oxigen contained in the green oxide by solution. The first, which I should have preferred, if circumstances had permitted me to adopt it, is, to dissolve a given quantity of iron in muriatic acid, and carefully to collect the hidrogen evolved; this measured, and for still farther certainty burnt in Volta's eudiometer, would give the quantity of oxigen combined with the iron. The second is

Two methods of doing this.

to

to dissolve a given quantity of iron in muriatic acid, and, after having precipitated it by an alkali, to dry it in an exhausted receiver by means of a lens.

Influence of the air on solutions of iron.

Oxigenation not the cause of the difference of the salts.

All the false notions diffused through the pneumatic theory of iron arise from having ascribed the colour and other properties, that distinguish the green and red salts, to a difference of oxigenation. This difference once established as a principle, nothing was more natural, than to refer to the same cause the transition of the salts of iron from green to red, on exposure to the air; particularly as the circumstances, that sometimes accompany this phenomenon, readily agree with this explanation.

Mistake of Scheele.

The authority of Scheele gave additional weight to this illusion. This celebrated chemist had observed, that, on dissolving green sulphate of iron in water, a sediment of green oxide commonly remained; and hence he inferred, that it was owing to the air contained in the water, which superoxided part of the green oxide; and this, becoming red, increased its saturating power with respect to the acid, and precipitated it. He then gave this process as a mean of ascertaining the quantity of air contained in water. Great as is the authority of this illustrious chemist, I must take the liberty of observing, that, even were this phenomenon owing to a superoxidation by means of the air contained in the water, this mode of appreciating the quantity would not be exact; because the quantity of precipitate does not depend solely on the red oxide formed by the air, but rather on the degree of acidity of the sulphate, which must always be supposed uniform according to Scheele's theory, but this is contradicted by experience. Thus admitting the superoxiding action of the air, a quart of water poured on a pound of very acid green sulphate would let fall no precipitate; while a quart of the same water would throw down a considerable quantity from another sulphate little or not at all acid.

The air in the water has no effect.

The explanation of this phenomenon too is defective in itself, for it takes place equally whether the water contain air, or be perfectly free from it. I made this comparative experi-

experiment with two equal quantities of distilled water, from one of which the air was completely expelled, while the other was saturated with it artificially, and the results were the same with both. If the crystals of sulphate I used were white, no deposition took place; but if they were green, a precipitate fell down, which was equally bulky in both solutions: so that Scheele's process is calculated rather to show the acidity of the green salts of iron to a certain point, than the quantity of air in the water.

The quantity of oxigen gas in the water must be far too small to produce the effect.

It may be objected to me, that, from the experiments of D. Carradori, boiled water always retains a little air: but, beside that I have lately repeated the experiment with water freed from air by Dr. Carradori's method with the same success, the experiments of Henry*, Humboldt, Gay Lussac, and more especially of Dalton†, on the absorption of gasses by water, completely terminate the controversy. According to Dalton, to whom I refer because he more directly turned his attention to this point, water saturated with atmospheric air contains only 2·012 per cent of its bulk, of which ·778 are oxigen gas, and 1·234 nitrogen. Consequently 100 cubic inches of water contain about $\frac{1}{3}$ of an inch of oxigen. Now if we consider, that the greater part of this gas is expelled by boiling; and if besides we make a correction for the heterogeneous substances, which according to Lambert and Saussure the air always contains, we shall find the influence of the oxigen contained in the water, even supposing it saturated with it, must be nothing; for if a green salt of iron with excess of oxide be thrown into 100 cubic inches of water at 60° [167° F.], a precipitate of at least 15 or 20 grains of red oxide will be formed, which cannot be attributed to the oxigen of the air contained in the water, unless the experiments of the learned natural philosophers I have quoted be altogether futile.

Proofs that the air does not oxigenate solutions of iron.

Beside this experiment; beside the ammoniacal solution of green sulphate, which passes to red without the possibility of suspecting the presence of oxigen; beside the precipitate, which did not increase its oxigenation 1 per cent by

* Philosophical Transactions for 1803, or our Journal, Vol. V. p. 229.

† Manchester Mem. N. S. Vol. I, or Journal, Vol. XIII. p. 291.

exposure

exposure to the air for a month; I have made several other experiments tending to the same object: and they have all convinced me, that the air has no superoxiding action on solutions of iron, at least at the common temperatures. Of these I shall recite only two, that are among the most conclusive.

Farther proofs.

1. I dissolved two equal portions of iron under circumstances perfectly similar. One of the solutions I put into a glass three inches in diameter, and immersed in it a curved tube, the extremity of which was a ball pierced with several holes. Through these I passed atmospheric air for seven hours at different times. At the expiration of three days, I compared these two solutions in various ways, and found, that the one into which I had forced air was perfectly similar to the other, which was not perceptibly altered, though the temperature was 12° [59° F.]. 2. By means of the same apparatus I passed about three quarts of oxigen gas through a solution of ten grains of iron, and, though the temperature was 25° [88°$\frac{1}{4}$ F.], it had no action on the solution.

On the colour of the green oxide.

Particles of precipitated oxide of iron hollow spheres containing a fluid.

On adding a few drops of alkali to a solution of iron a little diluted, I observed, that every particle of the oxide was formed of a very thin pellicle, including some fluid, and I accounted for the green colour by the difference of density between this pellicle and the fluid it enclosed. I likewise ascribed the alteration in green solutions exposed for some days to a temperature of 20° [77° F.] to the bursting of these vesicles by the dilatation of the fluid contained in them. To the pressure exerted upon these vesicles I attributed the unchangeableness of these solutions in bottles quite full and close stopped. But I could not reconcile with these modes of viewing the subject the change, that is induced in green solutions of iron by oxigenized muriatic acid, and in red solutions by sulphuretted hidrogen. The nature of the constituent principles of these two reagents renders the manner in which they are supposed to act in these two experiments so plausible, that I should not have withheld my assent to them, had I not been convinced by all the facts I have related,

This might account for their change of colour in some cases,

but not in others.

related, that oxigen has no influence in the red or green colour of oxides of iron. I meditated some experiments therefore, tending to observe the mode of action of these two reagents, when I recollected a fact observed at the beginning of my inquiry, which thus indemnified me for a number of errours, into which it had led me before.

White precipitate from iron.

In every kind of iron I had hitherto treated I found a substance, that fell down in a white precipitate, did not change by exposure to the air, gave an emerald green precipitate with prussiate of potash, and which I believe to be what Bergman called siderite, rather than phosphate of iron. On the other hand I obtained from some red salts of iron a white precipitate, that sometimes crystallized in laminæ very soft to the touch, which the most experienced mineralogist would take for French chalk, but which is nothing but a salt of iron with excess of oxide. I thought at the time, that these two substances were the same, that they were magnesia, and that this was nothing but iron at a maximum of oxidation. The name of this earth favoured the illusion; as did the opinion of former chemists respecting the transmutation of metals into earths. I deferred the investigation of this subject to a future period, making in the mean time only a few experiments on magnesia; and I found, that, on treating solutions of this earth with sulphuretted hidrogen, it afforded a precipitate similar to the green oxide of iron.

Supposed to be phosphuret of iron.

Sometimes resembling steatites.

Both imagined to be magnesia.

Sulphuretted hidrogen precipitates magnesia green,

Though my farther reserches concerning iron taught me, that neither of these substances was magnesia, the way in which this earth was coloured by sulphuretted hidrogen was a fact, the reason of which I intended to make the subject of future inquiry; and which, on recurring afresh to my memory, led me to suspect, that sulphuretted hidrogen might have a mode of action different from any with which we were acquainted. I was eager to repeat this experiment, not only on magnesia, but on lime and alumine likewise, and I found in fact, that the soluble salts of these three earths, treated by sulphuretted hidrogen, gave precipitates altogether similar to the green oxide of iron*. These precipitates exposed to

as it does lime and alumine.

* Certain management is necessary, to succeed in this experiment, which I have not repeated often enough, to be able to give certain instruction

The green colour goes off by exposure to the air.

to the air resume their white colour after some time, if they be not shaken; but agitation greatly accelerates this change, which is another similarity between these precipitates and those produced from the red salts of iron by sulphuretted hidrogen.

These green precipitates are hidrurets.

These green and earthy precipitates are not hidrosulphurets, as might be supposed; but hidrurets, which probably retain a little acid. This is proved by their being decomposed by oxigenized muriatic acid, without leaving any traces of sulphur; and by retaining their green colour, and other properties annexed to it, when redissolved in acids; which could not be the case if they were hidrosulphurets; since acids decompose these instantly.

The green oxides of iron are the same.

From these elucidations the cause of the green colour of oxide of iron, and its colouration by muriatic acid, suggested itself as it were spontaneously. The green oxide is never formed, unless hidrogen be set free: a part of this therefore remains engaged in the oxide, and imparts to it the green colour, with the property of being less soluble in water, or more crystallizable. This property of rendering a salt more crystallizable, possessed by a principle of so little density, appears at first sight inconsistent; but it is confirmed by the superoxigenized muriate of potash, which is rendered two or three times less soluble than the simple muriate by the addition of oxigen.

Oxigenized muriatic acid takes away their hidrogen.

Oxigenized muriatic acid then acts on a green salt of iron, as it does on sulphuretted hidrogen, phosphorus, &c. It deprives the oxide of the hidrogen with which it is combined, as it does the sulphur and the phosphorus: which at the same time proves, that in the oxide of iron it is not in the same state of dilatation as that in which we are acquainted with it uncombined, for in this state it does not combine with oxigenized muriatic acid at the temperature of the atmosphere.

The presence of hidrogen in the green ox-

The hidrogen likewise betrays itself in the offensive smell given out by a concentrated solution of iron, when a fixed

structions respecting it. I have left sulphuretted hidrogen in contact with sulphate of magnesia for an hour before it precipitated. Sometimes the precipitate is green in the very act of falling down; at other times it does not become green till some moments after.

alkali

alkali * is added to it, and the glass shaken a little: and we cannot allege, that this smell is owing to a few globules of hidrogen, that have remained mechanically entangled in the solution, since the same phenomenon takes place, if the solution be previously boiled. If the solution be diluted with six or eight parts of water at 50° or 60° [144° or 167° F.], and it be stirred with a glass rod as the alkali is put in, the smell is still very strong, and continues to exhale as long as an atom of green oxide remains in the precipitate; so that it is easy to tell by this, without seeing the precipitate, whether the solution be red or green. When a little green sulphate of iron in a very concentrated solution, like that which gives a white precipitate, is precipitated in a phial, and this is corked and shaken, it will be seen, that the volume of gas is increased; for, if the cork do not fit very tight, it will be forced out, notwithstanding the temperature continues the same. If the air in the phial be afterward examined, it will be found sometimes to extinguish a candle, or to detonate on its application. Now both these are compatible with the presence of hidrogen, according as it is pure or mixed; and possibly there may be a little iron dissolved in it, as zinc or arsenic sometimes is.

ides of iron proved by the smell,

and by the gas evolved.

To satisfy myself still farther of the presence of hidrogen, and its influence on the salts of iron, I adapted to a tubulated retort a small receiver, and to this a curved tube, the extremity of which opened under a jar in the pneumatic apparatus. Into the retort I poured a solution of green sulphate of iron recently made, having previously boiled it half an hour, to prevent any suspicion of hidrogen mechanically retained in it. This solution I precipitated with caustic soda greatly diluted with boiling water. As soon as the mixture was brought to boil, a gas fetid as hidrogen was evolved, which detonated on the contact of flame. The water of the pneumatic apparatus too had the nauseous taste and smell of hidrogen disengaged from solutions of iron.

Farther proof by expelling the hidrogen.

In order to expel all the hidrogen, or to convert all the oxide from green to red, I continued the distillation.

* If ammonia be used, its smell conceals that of the hidrogen.

Scarcely

Scarcely was the mass dry, when the retort burst, and I found in it more than 300 grains of red oxide, with a little green oxide, which occupied the bottom of the retort. The pressure of the red oxide and sulphate of soda, by which the green oxide was covered, had prevented the disengagement of the hidrogen from this portion.

Proof that green oxide of iron does not acquire more oxigen from oxigenized muriatic acid.

In support of my opinion I shall add two facts, which, though less direct than those I have already given, are of considerable weight. When oxigenized muriatic acid is poured on a green solution, if the oxigen of the acid combined with the oxide of iron, a considerable extrication of caloric must take place, in consequence of its more dilated state in the acid, and more condensed or fixed state in the oxide, from which the greatest heat of our furnaces cannot expel an atom. But I convinced myself by several experiments, that the increase of temperature is scarcely perceptible. This slight evolution of caloric is consistent with the combination of the hidrogen, given out by the hidruret of iron, and the oxigen from the muriatic acid, because in both these combinations the gasses are nearly in the same state of dilatation as when they form water.

Proof that sulphuretted hidrogen does not deprive it of oxigen.

Lastly, if the action of sulphuretted hidrogen on a red solution of iron be merely to bring it back to the same degree of oxidation, as that in which the common green solutions are, the properties of both should be the same. On the contrary, the solutions rendered green by sulphuretted hidrogen rapidly change red on exposure to the air; and heated for a quarter of an hour they become entirely red: which is not the case with the common green salts recently made.

(To be concluded in our next.)

VI.

Mr. Curwen's Weed Harrow.

Fig. 1.

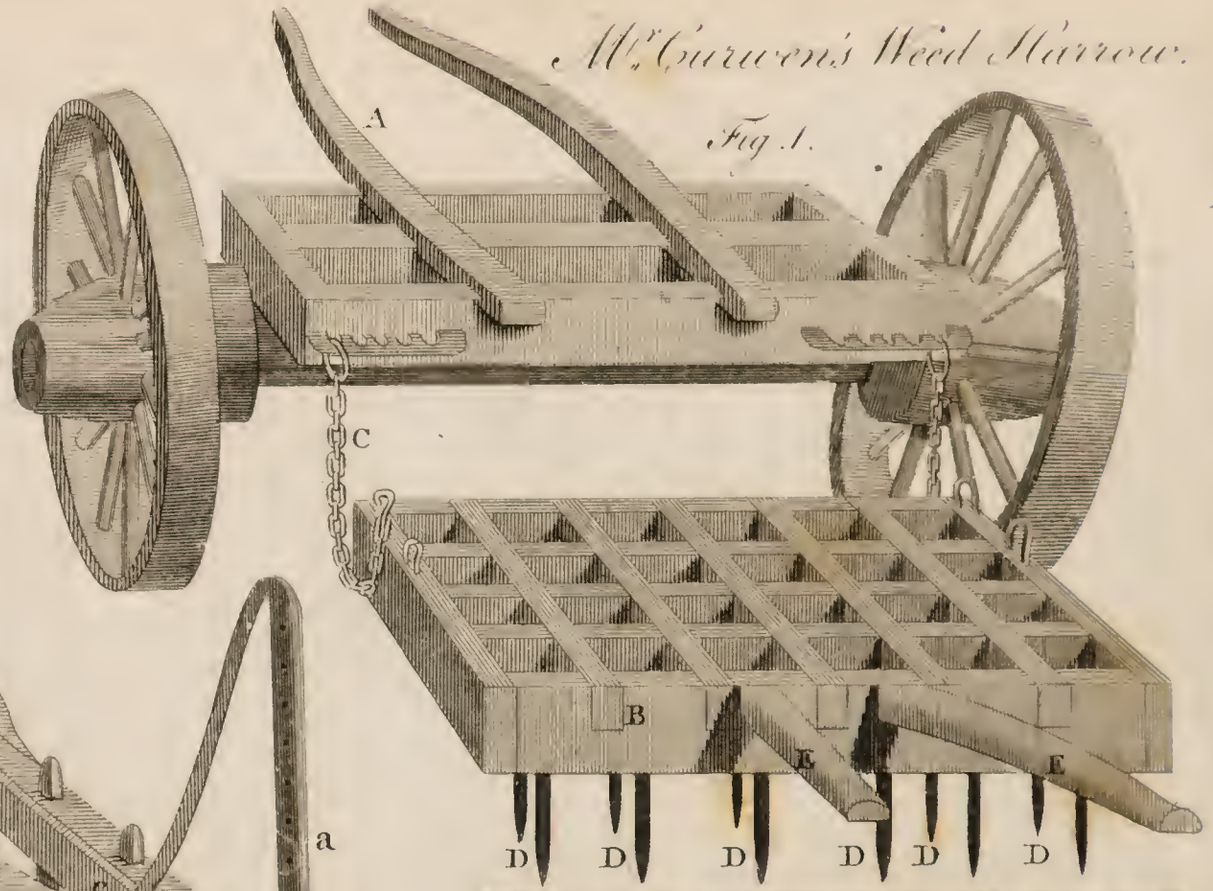
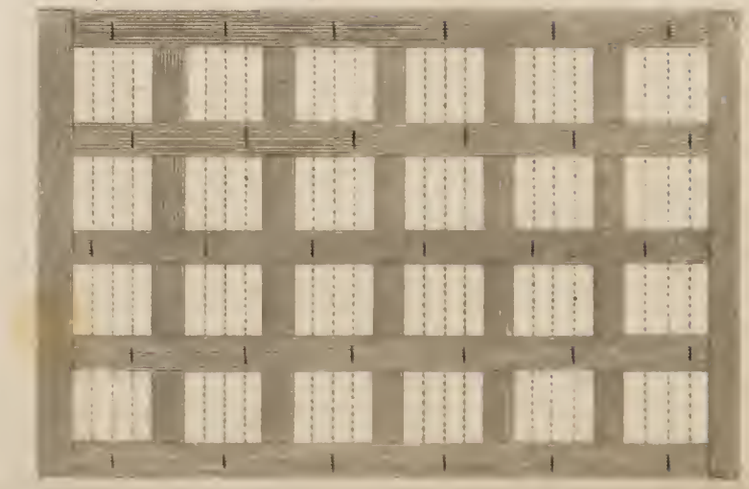


Fig. 2.



Mr. Waistell's Horse Hoe.

Fig. 3.

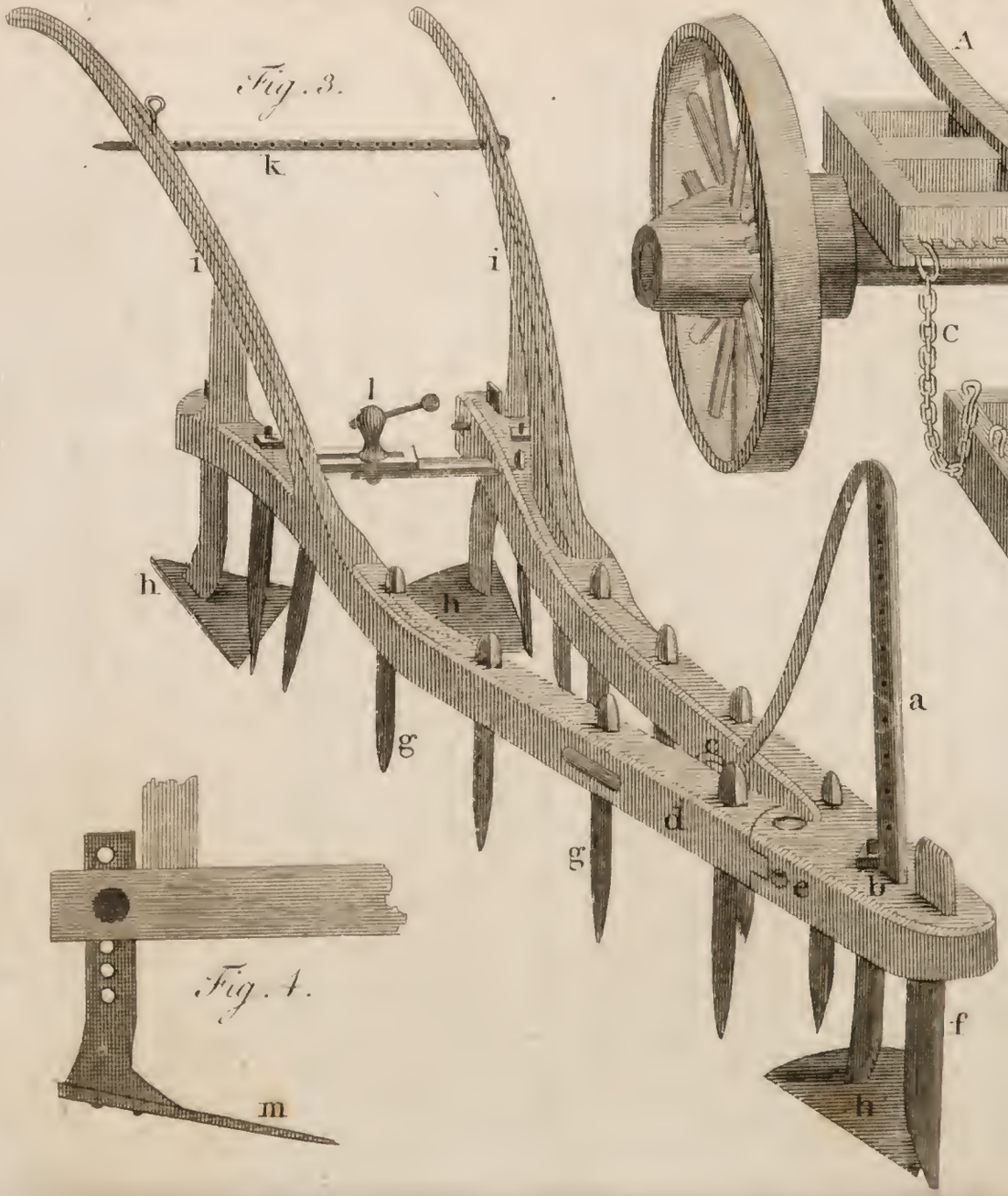


Fig. 4.

V.

Account of Mr. CURWEN's *Drill Horse Hoe, or Weed Harrow.**

SIR,

AS one great and most important advantage of drill husbandry proceeds from the opportunity of cleaning foul grounds, as also of breaking and loosening stiff soils, to give the power of extension to the roots of grain; whatever can facilitate these operations, will, I flatter myself, be deemed worthy of the attention of the Society. Advantage of drill husbandry.

Having hitherto found great difficulty and much labour necessary in accomplishing the cleaning of wheat and other grain, I have been led to make some experiments, and I am sanguine in my hopes, that the harrow I send for the inspection of the Society will be found to accomplish the purpose with greater ease and facility, than any thing at present in use. Difficulty of cleaning from weeds.

The simplicity and ease, with which it is worked, have enabled me, this season, to give my wheat crop, which exceeds one hundred acres, two cleanings, and at an expense of somewhat less than a shilling per acre each operation; a man and boy, with one horse, being able to clean above seven acres a day. The direction of the harrow, to prevent its injuring the grain, is effected by an alteration of the chain, by which it is attached to the wheels. The distance of the teeth from the centre tooth must be regulated by the width of the drills. In case they exceed a foot, the harrow should be broader, to admit of another row of teeth. To clean at nine inches, two inches and a half are allowed on each side of the centre tooth, by which means every part of the earth is cut between the rows of grain. The size and strength of the teeth must be regulated by the nature of the soil. The thing is so simple, that I hesitated laying it before the Society, till I was encouraged by persons, whose experience and knowledge are infinitely greater than my own. Harrow for this purpose.

The complete introduction of drill husbandry would, I conceive, be of great national importance, and under this Drill husbandry recommended.

* Transactions of the Society of Arts, for 1806.

conviction whatever can facilitate its operations may not be unworthy of attention, and will, I hope, excuse the liberty I have taken.

I have the honour to be, Sir,

Your obedient humble Servant,

J. C. CURWEN.

London, May 5th, 1806.

Explanation of the Engraving of Mr. Curwen's Drill Horse-Hoe, or Weed Harrow. Plate VIII. *Fig.* 1, 2.

Explanation of the plate.

Fig. 1. shows the carriage, within the shafts of which, A, the horse is placed: the carriage wheels are intended to be half the width of the butts or stitches, so that once going up, and once returning, will be sufficient to clear each butt from weeds.

B, The hoe, or harrow, which is attached to the carriage by the chains C C. The harrow may be raised higher, or sunk lower, or placed more on one side or another, as occasion may require, by altering the position of the chain, as will appear by an inspection of the plate.

D D D D D D, Six double rows of teeth, or knives, which are so placed in the frame, that each double row may pass up the interval betwixt the rows of corn, and cut or pull up the weeds, that grow in such intervals, without injuring the corn. These knives are strong, and have a sharp edge in front.

E E Are the two handles, by which the person who holds them may direct the knives, or teeth of the harrow, to pass in straight lines up the intervals.

Fig. 2. shows the underside of the weed harrow, that the positions of the double rows of the knives, and of the space left to prevent the corn being injured, may be more clearly seen.

Certificates in Favour of Mr. Curwen's Harrow.

The harrow clears all the weeds,

At the request of J. C. Curwen, Esq., I beg leave to state, that I have been present, when Mr. Curwen's harrow for cleaning drilled corn has been used, and have worked a little with it myself; that its effect appeared to me most highly beneficial in clearing away in the spring all the

weeds.

weeds, that had grown during the winter among the wheat, without the least injury to the grain; and also in raising up the top soil, which had become sad and heavy, and thus enabling the spring shoot to take root more easily: and at the same time it covers the roots of the corn with fresh soil, which are often left quite bare by the washing of the rains in winter, and so subject to be killed by the frost. It also enables the farmer, to sow his barley much earlier than he could broad-cast, as it will both clear the corn previous to sowing the grass seeds, and afterward harrow them in. Its utility in every respect appeared to me so very great, that I was induced to adopt the plan of sowing my corn with the drill upon my fallows this spring, and have accordingly got a harrow made upon the model of Mr. Curwen's.

raises the top soil,

covers the roots of the corn,

and harrows in grass seeds.

I have the honour to be, &c.

J. D. B. DYKES.

Dovenby Hall, May 13, 1806.

SIR,

We whose names are hereunto subscribed do certify, that we have paid particular attention to a harrow, made use of in the farm of J. C. Curwen, Esq., for the purpose of harrowing between the rows of drilled grain. We conceive it of great utility; the expedition is undeniable, as upwards of seven acres can be done with ease in eight hours, with only a boy to lead the horse, and a man to regulate the harrow.

We are, Sir,

Your most obedient humble Servants,

Thomas Gaff, *Merchant*, Workington Hall Mills.

Matthew Foster, *Farmer*, at Calva, near Workington.

Workington-Hall, May 19, 1806.

VI.

Account of a Drill Horse Hoe for Turnips, communicated by Mr. Charles Waistell.*

Drilling turnips decidedly preferable to broad-cast.

In consequence of the premiums, which the Society of Arts has offered, and bestowed for several years, on the comparative culture of turnips, the drill practice has appeared so decidedly superior to the broad-cast method, that they have thought it unnecessary to continue them. At the same time they have given a figure and description of a useful drill hoe and harrow, with which they observe they shall probably finish the subject, and which therefore we shall lay before our readers nearly in Mr. Waistell's words.

Dear Sir,

Hoe harrow for turnips and other wide drilled crops.

I have ordered a new agricultural implement to be left for a short time at the Society's Repository for inspection. It is called a hoe harrow, and is, as its name imports, a hoe and harrow combined. For destroying the weeds, and pulverising the soil in the intervals of drilled turnips, and of other crops drilled sufficiently wide to be horse-hoed, I know not of any other implement of equal efficacy.

Its use.

It enables the farmer to cultivate those intervals as completely as a well wrought fallow, so long as the horse can travel therein, without injury to the growing crop. I know not who the meritorious inventor is. The first I saw was a few years ago at West Park, near Barnard Castle. This was brought from Carlisle by my brother, and many have been made from that pattern, and are now in use, and are highly approved of by farmers in the neighbourhood of Barnard Castle, where the turnip crops are now generally raised in drills about 27 inches apart. This mode was first introduced there about 23 years ago, before which time they were all sown broad-cast.

Near Barnard Castle turnips drilled at 27 inches.

An implement of husbandry, possessing such superior utility, as this hoe harrow seems to me to possess, is deserving of being made known as generally and as speedily as possible. I conceive this would be best effected through the

* Trans. of the Soc. of Arts for 1806.

medium

medium of your respectable Society, to whose notice I must entreat you to have the goodness to introduce this implement. Should they concur in opinion with me respecting it, I am persuaded, that they will give a plate and description of it in their next volume.

Convinced, that the fertility and productiveness of our arable grounds may be much increased by a more general practice of the horse-hoeing husbandry, I wish to see the practice of it advanced more nearly to perfection, as that must tend to promote its more general adoption. Horse hoeing recommended.

I am, Dear Sir,

Your very humble Servant,

CHARLES WAISTELL.

No. 99, *High Holborn.*

Explanation of the Engraving of Mr. Waistell's Drill Horse Hoe Harrow, Plate VIII. *Fig.* 3.

Fig. 3. shows the hoe harrow, to which the horse is to be attached by the upright iron *a*, in which are a number of holes, to admit the drag chain to be put higher or lower, as may be found necessary. This iron is at one end fixed firm in the fore part of the machine at *b*, and at the other end to the farther side, or wing, *c*. Explanation of the plate.

d, Is the nearer side or wing of the machine, and movable by a joint at *e*. This wing may by this mean be expanded or contracted, as the interval between the rows to be cleared of weeds may require.

f, A strong wedge-like tooth in the fore-part of the machine, to tear up the weeds, which are deep in the ground.

g, *g*, Other teeth more slender, fixed in the two wings or sides of the machine, and also intended to tear up weeds and loosen the earth.

h, *h*, *h*, Three triangular hoes. That which is in front has a strong iron fixed in its centre; the two others at the hinder part of the machine have the irons fixed at the farther corner of each. The intent of the centre hoe is to cut off the weeds in the middle of the interval; and of the other two, those on each side next the crop, and to lay all the weeds in a ridge-like form in the middle of the path, to dry and rot.

i, *i*, The

i, *i*, The two handles, by which the machine is managed.

k, A slender iron bar, with a peg and holes to direct the distance of the expansion or contraction of the machine.

l, A strong iron vice, which works in a grooved iron, fixed to the inner side of the wing *d*, and which, when screwed down, holds the machine firm at the distance of expansion wanted for use.

Fig. 4. Shows on a larger scale one of the hinder hoes separate from the machine, and the manner by which it may occasionally be raised or lowered in the machine by a pin and holes.

VII.

On Capillary Action. By Mr. Laplace.*

Results of capillary attraction generalized.

By considering the theory of capillary action in a new point of view, I have not only succeeded in simplifying it, but in generalizing the results, to which I had been led before by analysis. I had determined the elevation or depression of fluids only in circular capillary spaces, and between planes: but I shall here proceed to determine them, whatever these spaces may be, or whatever the nature of the surfaces by which they are included; supposing even in these spaces any number of fluids placed one above another; and I shall thence deduce the increase or diminution of weight, that bodies plunged in fluids undergo by capillary action.

Determined for spaces of any figure, and for any number of fluids.

Affinities of substances to fluids deduced from their resistance to separation.

The combination of these results with those I have found by analysis has given me an accurate expression of the affinities of different substances to fluids, by means of experiments made on the resistance, that disks of different substances, applied to the surface of fluids, oppose to their separation. I dare venture to believe, that this will throw great light on the theory of affinities; for what I advance is founded on geometrical reasonings, and not on vague and precarious considerations, which ought to be strictly banished from natural philosophy; unless, imitating Newton in his Optics, we give them merely as conjectures calculated

* Journal de Physique, Vol. LXIII. p. 474, Dec. 1806.

to

to guide us to farther searches, but leaving the merit of discovery almost wholly to him, who shall establish them on solid foundations by observation or analysis.

Supplement to the Theory of Capillary Action promised.

I intend to publish without delay, in a supplement to my Theory of Capillary Action, the analytical demonstrations of the theorems, which I have only mentioned in different numbers of this Journal. At the same time I shall give a new method of arriving at the fundamental equations of this theory. From these equations I shall deduce the general theorems, which I am now about to lay before the reader, demonstrating them by the direct consideration of all the forces, that concur in the production of capillary effects. It will appear, that the forces, on which these effects depend, do not stop at the surface of fluids; but that they extend through the whole of their interior, and even to the extremities of the bodies immersed in them; which establishes the complete identity of these forces with affinities.

Forces on which capillary attraction depends not confined to the surface, and identical with affinities.

Theorem.

"If we conceive any kind of prismatic tube, in a vertical position, with its inferior extremity immersed in a fluid of indeterminate quantity; the volume of fluid within, raised above the level by capillary action, is equal to the circumference of the interior base of the prism, multiplied by a constant quantity, which is the same for all prismatic tubes of the same matter immersed in the same fluid."

Demonstration.

To demonstrate this theorem, let us imagine, at the inferior extremity of the tube, a second tube, the infinitely thin sides of which are the prolongation of the interior surface of the first tube, and, having no action on the fluid, do not prevent the reciprocal attraction between the molecules of the first tube and the fluid. Let us suppose, that the second tube is at first vertical, that then it bends horizontally, and that afterward it resumes its vertical direction, retaining the same figure, and the same size, throughout its whole extent. It is evident, that, while the fluid is in equilibrio, the pressure in the two vertical branches of the canal formed by the first and second tube will be the same. But, as there is more fluid in the first vertical branch formed of the first tube and part of the second, than in the other vertical branch, the excess of pressure, that results from this, must be destroyed by the attractions of the prism and the fluid

for

for the fluid contained in this first branch. Let us analyse these different attractions with care, and first consider those that take place toward the lower part of the first tube.

Attractions toward the bottom of the tube.

For this let us conceive, that the base of the tube is horizontal. The fluid contained in the second tube will be attracted vertically toward the bottom, 1st by itself, 2dly, by the fluid surrounding the second tube. But these two attractions are destroyed by the similar attractions, that the fluid contained in the second vertical branch of the canal experiences near the surface of the level of the fluid. Accordingly we may leave them out of consideration here.—The fluid in the first vertical branch of the second tube will also be attracted perpendicularly upward by the fluid in the first tube. But this attraction will be destroyed by the attraction it exerts on the latter fluid: these two reciprocal attractions therefore may be set aside. Lastly, the fluid in the second tube will be attracted perpendicularly upward by the first tube, and hence this fluid will have a vertical force, which we shall denote by Q, that will contribute to destroy the excess of pressure owing to the elevation of the fluid in the first tube.

Forces acting on the fluid in the tube.

Let us now examine the forces, with which the fluid in the first tube is actuated. In its lower part it experiences the following attractions: 1st, it is attracted by itself; but the reciprocal attractions of the particles of a body impress upon it no motion, if it be solid; and we may conceive the fluid in the first tube to be consolidated, without any disturbance of equilibrium. 2dly, This fluid is attracted by the fluid in the interior of the second tube: but we have seen, that the reciprocal attractions of these two fluids destroy each other, and must not be taken into account. 3dly, It is attracted by the exterior fluid, that surrounds the second tube; and from this attraction results a vertical force acting downward, which we shall denote by $-Q'$. We prefix to this the sign $-$, to indicate, that its direction is contrary to that of the force Q. We shall observe here, that, if the laws of attraction relative to the distance be the same for the molecules of the first tube and those of the fluid, so that they differ only in respect to intensity; if we nominate these intensities in equal volumes ε and ε', the forces Q and Q' are

proportional to ϱ and ϱ': for the interior surface of the fluid surrounding the second tube is the same as the interior surface of the first tube, so that the two masses differ only in their thickness. But as the attraction of masses becomes insensible at sensible distances, the difference in their thicknesses can produce none in their attractions, provided these thicknesses be sensible. 4thly, and lastly, The fluid of the first tube is attracted vertically upward by this tube. In fact let us conceive this fluid divided into an infinite number of little vertical columns: if we draw a horizontal plane through the superior extremity of one of these columns, the part of the tube below this plane will produce no vertical force in that column. No vertical force will be produced therefore, but what is owing to the part of the tube above the plane; and it is evident, that the vertical attraction of this part of the tube for the column will be the same as that of the whole tube on an equal and similar column placed in the second tube. Thus the whole vertical force produced by the attraction of the first tube on the fluid it contains will be equal to that, which the attraction of this tube produces on the fluid contained in the second tube: this force therefore will be equal to Q.

Vertical force acting upward.

On combining together all the vertical attractions experienced by the fluid contained in the first vertical branch of the canal, we shall have a vertical force directed upward, and equal to $2Q-Q'$. This force must balance the excess of the pressure arising from the weight of the fluid raised above the level. Let V be its volume, D its density, and g its specific gravity, $gD\times V$ will be its weight. Thus we shall have $gD\times V=2Q-Q'$.

The base may be considered as a plane.

Now attraction being sensible only at imperceptible distances, the first tube acts sensibly only on columns extremely near to its sides: we may neglect the curvature of these sides therefore, and consider them as developed on a plane surface. The force Q will be proportional to the magnitude of this surface; or, which comes to the same thing, to the circumference of the base of the interior surface of the parallelopipedon. Thus, if we call this circumference c, we shall have $Q=\varrho\times c$; ϱ being a constant proportional to the intensity of the attraction of the matter of the first tube for the

the fluid. We shall also have $Q'=\varrho' \times c$; ϱ' being proportional to the intensity of the attraction of the fluid for itself. Therefore $V=\frac{(2\varrho-\varrho')\times c}{g\,D}$; which is the algebraic expression of the theorem to be demonstrated.

The constant quantity $\frac{2\varrho-\varrho'}{g\times D}$ may be determined by means of the observed elevation of the fluid in a very narrow cylindrical tube. Let q be the height, to which the fluid rises in this tube, and l the radius of its cavity: putting π for the semicircumference of which the radius is unity, we shall have nearly $V=\pi\times l^2 q$, $c=2\,l\,\pi$: the preceding equation then will give $\frac{2\varrho-\varrho'}{g\times D}=\frac{l\,q}{2}$, and consequently we shall have $V=\frac{l\,q}{2}\times c$.

If ϱ' exceed 2ϱ, q will be negative; and consequently, the elevation of the fluid changing to depression, V will be negative.

Let us put h for the mean height of all the fluid columns, that compose the volume V, and b for the interior base of the parallelopipedon: then we shall have $V=h\,b$, and consequently $h=\frac{l\,q\times c}{2\,b}$.

Proportions of bases, if similar figures.

When the bases of different parallelopipedons are similar figures, they are proportional to the squares of their homologous sides, and their circumferences are proportional to these sides.

If regular polygons.

If these bases be regular polygons, they will be equal to the products of their circumference multiplied into half the radius of the inscribed circle: the heights h therefore will be reciprocals to these radii. Denoting these radii by r, we shall have $h=\frac{l\,q}{r}$.

A square and a triangle.

Thus supposing two equal bases, one of which is a square, and the other an equilateral triangle; the values of r will be to each other as 2 to $3^{\frac{3}{4}}$, or nearly as 7 to 8.

The law confirmed in these by Gellert.

Mr. Gellert has published some experiments on the elevation of water in rectangular and triangular prismatic tubes of

of glass, in the Memoirs of the Petersburg Academy, vol. XII. These confirm the law, according to which the heights are reciprocals to the homologous lines of similar bases. Mr. Gellert farther concludes from his experiments, that the elevations of a fluid are the same in rectangular and triangular prisms, the bases of which are equal. But he admits, that this is not so certain as the law of the heights being reciprocal to the homologous lines of similar bases. In fact it has just been seen, that there is a difference of an eighth between the elevations of the fluid in two prisms, the bases of which are equal, and one of which is a square, the other an equilateral triangle. The experiments related by Mr. Gellert do not afford sufficient data, to compare their results exactly with the preceding theory.

A narrow parallelogram.

If the base of the parallelopipedon be a rectangle, the larger side of which is equal to a, and the other side, supposed very small, equal to l, we shall have $b=a\,l$, and $c=2\,a+2\,l$: consequently $h=\frac{l\,q\times(l\,a+2\,l)}{2\,a\,l}=q\times\left(1+\frac{l}{a}\right)$; and by neglecting $\frac{l}{a}$ we shall have $h=q$, agreeable to experience.

Where several fluids are concerned.

"If the indefinite vessel, in which the parallelopipedon is immersed, include any number of fluids placed horizontally one above another; the excess of the weights of these fluids contained in the tube, over the weight of the fluids which it would have contained without capillary action, is the same as the weight of the fluid that would rise above the level, if the vessel contained only that fluid in which the inferior extremity of the parallelopipedon is immersed."

In fact, the action of the prisms and this fluid on the same fluid included in the tube, is evidently the same as in the latter case. The other fluids contained in the prism being raised sensibly above its base, the prism has no action on either of them to raise or depress it. As to the reciprocal action of these fluids on one another, it would evidently be destroyed, if they formed a solid mass together, and this we may suppose without any disturbance of equilibrium.

Case of two fluids.

"If the vessel contain but two fluids, in which the prism is entirely immersed, so that its superior part is in one

one fluid, and its inferior in the other; the weight of the lower fluid, raised in the prism by capillary action above its level in the vessel, will be equal to the weight of a similar volume of the upper fluid, *plus* the weight of the inferior fluid, that would rise in the prism above the level, if there were no other fluid in the vessel, *minus* the weight of the superior fluid, that would rise in the same prism above the level, if the vessel contained this fluid only."

Demonstration. To demonstrate this, it is to be observed, that the action of the prism on the portion of the inferior fluid it contains is the same as if this fluid only were in the vessel: in both these cases then this fluid is drawn perpendicularly upward in the same manner, both by the attraction of the prism, and that of the fluid that surrounds the lower part of the prism; and these attractions united are equivalent to the weight of the volume of this fluid, that would ascend in the prism above the level, if it were alone in the vessel. In like manner the superior fluid, contained in the upper part of the prism, is drawn perpendicularly downward by the attraction of the prism and the fluid that surrounds this part, as it would be drawn downward by the same attractions, if the vessel contained only the superior fluid; and these attractions united are equivalent to the weight of the superior fluid, that would then rise in the prism above its level in the vessel. Lastly the column of fluids within the prism, which is above the level of the inferior fluid in the vessel, is drawn perpendicularly downward by its own weight, and perpendicularly upward by the weight of a similar column of the superior fluid. On combining all these forces, which must counterbalance each other, we shall have the theorem just announced. By the same principles we may determine what will take place, when a hollow prism is entirely immersed in a vessel filled with any number of fluids.

Where the base of the prism is not horizontal. In what has been said the base of the prism was supposed to be horizontal: but if it were inclined to the horizon, the vertical action of the prism on the fluid would still be the same. For a plane of a sensible thickness, having its lower part, the surface of which is terminated by a right line inclined to the horizon, immersed in a fluid, attracts this

this fluid parallel to its surface, and perpendicularly to the right line that terminates it, proportionally to the length of this line: but this attraction, resolved into a vertical force, is proportional to the horizontal magnitude of the plane. Hence it is easy to conclude generally, that, whatever be the figure of the base of the prism, its vertical attraction, and that of the exterior fluid on the fluid included in it, are the same as if the base were horizontal. The first theorem therefore will hold generally, if we understand by the circumference of the interior base that of the interior section, perpendicular to the sides of the prism.

The volume of fluid raised above the level in the inverse ratio of the sine of the inclination.

" If the prism, the lower part of which is immersed in a fluid in a vessel of indefinite size, be inclined to the horizon, the volume of fluid in the prism raised above the level of the fluid in the vessel, multiplied by the size of the angle of inclination between the side of the prism and the horizon, will be constantly the same, whatever this inclination may be."

In fact, this product expresses the weight of the volume of fluid raised above the level, and resolved into a force parallel to the sides of the prism: this weight, thus resolved, must balance the attraction of the prism and the external fluid to the fluid it contains; an attraction evidently the same, whatever may be the inclination of the prism; therefore the mean perpendicular height of the fluid above the level is constantly the same.

Ascent of a fluid between two parallelopipedons of the same material.

" If a parallelopipedon be placed perpendicularly in another parallelopipedon of the same material, and their inferior extremities be immersed in a fluid; putting V for the volume of fluid raised above the level in the space included between the two parallelopipedons, we shall have $V = \frac{(2\varsigma - \varsigma')}{g \times D} \times \left(c + c'\right) = \frac{l\, q^2}{2} \times \left(c + c'\right)$; c being the inner circumference of the base of the larger parallelopipedon, and c' the outer circumference of the base of the smaller."

Demonstrated.

This theorem is demonstrable in the same manner as the first. If the bases of the two parallelopipedons be similar polygons, the homologous sides of which are parallel, and placed all at the same distance, if we put l for this distance, the

the base of the space the two parallelopipedons leave between them will be $\frac{l \times (c+c')}{2}$: thus, h being the mean height of the fluid raised, we shall have $V=h\,l \times \frac{(c+c')}{2}$, and consequently $h=q$. We may determine too from the preceding principles what will take place, if the prisms be immersed wholly or partly in a vessel filled with any number of fluids, and in the case of their being inclined to the horizon.

Where they are of different materials.

" The data being the same as in the preceding theorem, if the two parallelopipedons be of different materials, put ϱ for the force of attraction which that of the greater has for the fluid, and ϱ_1 for the attractive force of that of the smaller, we shall have, $V=\frac{(2\varrho-\varrho')}{g\,D} \times c+\frac{(2\varrho_1-\varrho')}{g\,D} \times c'$: so that, if we put q and q_1 for the elevations of the fluids in two very narrow cylindrical tubes of the same interior radius l, formed of these two materials respectively, we shall have $V=\frac{1}{2} l \times (qc+q_1 c.")$

This theorem too is demonstrable in the same manner as the first. It is easy to perceive, that by the same principles we shall obtain the volume of fluid raised above the level in a space included between any number of vertical planes of different materials.

Attraction on the outside of a prism.

It follows from the preceding theorem, that the volume V of the fluid raised by capillary attraction exteriorly to a prism immersed in a fluid at its inferior extremity, is equal to $\frac{2\varrho-\varrho'}{g\,D} \times c=\frac{1}{2} l\,q \times c$; c being the outer circumference of the prism. The increase of weight of the prism, owing to capillary attraction, is equal to the weight of this volume of fluid. It changes to diminution, if q be negative, and then the prism is raised by capillary action. If the base of the prism be a very narrow rectangle, of which a is the longer side, and l the shorter, putting i for its height, its solidity will be $a\,i\,l$, and its circumference, c, will be $2\,a+2\,l$; and the volume V of fluid depressed by capillary action will be $a\,q\,l \times \left(1+\frac{l}{a}\right)$. Putting k then for the

Hence increase of weight, or diminution,

ratio

ratio of the specific gravity of the prism to that of the fluid, the weight of the prism will be to that of the volume of fluid depressed as $i\,k : q \times \left(1 + \frac{l}{a}\right)$. By suitably diminishing i therefore, we may render the two weights equal, and thus keep the prism at the surface of the fluid. From the preceding principles too we may determine the diminution of weight of a body completely immersed in a vessel filled with several fluids. or equilibrium.

If the end of a very slender tube be immersed perpendicularly in a fluid, putting l for the radius of the cavity of the tube, and q for the height to which the fluid is raised above the level in it, we shall have, by my theory of capillary action, $l\,q = \frac{\cos.\ \varpi}{\alpha\ D}$; ϖ being the angle which the surface of the interior fluid forms with that part of the inner surface of the tube, which is in contact with it. When the fluid is depressed below the level, this angle exceeds a right angle, and then its cosine becomes negative, as well as q: but α is a constant quantity, which depends only on the weight and action of the fluid on itself. By what precedes we have, $\frac{2\,\varrho - \varrho'}{g\,D} = \frac{l\,q}{2}$: therefore we shall have $\cos.\ \varpi = \frac{2\,\alpha \times (2\,\varrho - \varrho')}{g\,D}$; (1.)

But it has appeared in the theory quoted, that, ϱ being null, ϖ is equal to two right angles: which may be concluded likewise from the analysis I shall give in a supplement to that theory, on the resistance that a very large circular disk, applied to the surface of a fluid, opposes to its separation from the fluid. From this analysis it follows, that, i being the radius of the disk, supposed of the same matter as the preceding tube, this resistance is equal to $\frac{g\,D \times \pi \times i^2 \times \sqrt{2} \times \cos.\ \frac{1}{2}\varpi}{\sqrt{\alpha}}$: but it is clear, that it must be null, when ϱ is null, or when the disk has no action on the fluid; we shall then have $\cos.\ \frac{1}{2}\varpi$ null, which gives $\varpi = 2\pi$, and consequently $\cos.\ \varpi = -1$: thus the equation (1) will give $\varrho' = \frac{g \times D}{2\,\alpha}$, and consequently $\frac{\varrho}{\varrho'} = \cos.^2 \times \frac{1}{2}\varpi$. Resistance a disk opposes to separation from a fluid.

Hence

Hence the preceding expression of the resistance the disk opposes to its separation from the fluid, or, which comes to the same thing, of the weight necessary to raise it, becomes $2\pi \times i^2 \times \sqrt{g D \times \varsigma}$. "For disks of the same diameter therefore, and different substances, the squares of these weights, divided by the specific gravities of the fluids, are proportional to the value of ς." Accordingly, by very accurate experiments on the resistances opposed by disks to their separation from the surfaces of fluids, we may determine their respective attractions for those fluids.

Attraction of a substance for a fluid determinable from its adhesion to the surface.

Two important observations are here to be made: the first is, that ς expresses the action of a plane of a sensible thickness on a fluid plane of a sensible thickness parallel to it, and touching it by the right line, that terminates one of its extremities; whatever be the laws of the attraction of the molecules of the fluid for those of the plane, and for each other, even in the case where these laws are not expressed by the same function of the distance. But if this function be the same, then the values of ς and ς' are proportional to the respective intensities of the attractions; or, which comes to the same thing, to the constant coefficients, which multiply the common function of the distance, by which the law of these attractions is represented; but these values are relative to equal volumes.

To show this, let us conceive two capillary tubes of the same diameter and different substances, but in which a fluid rises to the same height. It is clear, that, if in these tubes we take two equal volumes, infinitely small, and similarly placed, with respect to the interior fluid, their action on this fluid will be the same, and one may be substituted for the other. But to have their attractions in equality with the masses, the attractions of equal volumes must be divided by the specific gravities: the values of ς and ς' therefore must be divided by the respective densities of the different substances.

The second observation is, that the preceding results suppose ς less than ς': for, if ς exceeded ς', the fluid would unite intimately with the disk with which it was in contact, and thus form a new disk, the surface of which in contact with the fluid would be the fluid itself. But as by the preceding formula we may determine the resistance, that such a disk

a disk would oppose to its separation; we may be certain, that ϱ is less than ϱ', if the resistance opposed by a disk be less than the resistance thus calculated.

XIV.

Letter from Mr. DELAVILLE, *M. D. of Cherbourg, to Mr. Vauquelin, Member of the Institute, on the Oxidation of Metals, and particularly on that of Lead*.*

SIR,

I HAVE undertaken, and pursued as far as my occupations would allow me, some experiments on the oxidation of metals, particularly on that of lead; and though the results I have obtained are such as to inspire me with a wish to push my inquiry still farther, as the publication of these results, which I conceive to be not yet known, at least to many, may throw some light on the theory of oxidation in general, and contribute to render more economical the oxidation of lead in particular, as well as the preparation of some salts, that have this metal for their base, I shall do myself the honour of sending you a short account of these results, and of the means I adopted to obtain them. If like me you think them new, at least in some respects, I beg you would give them that sort of publicity, that may appear to you most suitable. Experiments on oxidation of lead.

It is known, that, in cleaning bottles, when a small quantity of shot is shaken in water, the friction in a short time separates particles of lead, which, being suspended in the water, render it turbid, and give it a slate gray colour. If the agitation be carried farther, the particles suspended in the water become of a lighter gray; and by continuing it they grow whitish, and at length of a pretty fine white. Shot oxided in washing bottles.

This oxide of lead has such a tendency to unite with carbonic acid, that on being exposed to the air, when taken out of the air, it is covered almost immediately with The oxide powerfully attracts carbonic acid.

* Annales de Chimie, Vol. LXVIII. p. 92.

a pellicle of a brilliant white, which appears to be nothing but carbonate of lead.

May be kept under water unchanged,

If kept under water, this oxide of lead undergoes no perceptible change, whether it be exposed to the light, or defended from it. But if it be kept ever so little time in a flint glass phial with a little water only, it is found to attach itself to the sides of the phial above the surface of the water; and if the phial be exposed to the rays of the sun, that portion of oxide acted upon by the light changes successively from white to yellow, and from yellow to red, thus furnishing massicot and minium.

but exposed to air and light becomes yellow, and then red.

Changed to massicot and minium by heat.

If the white oxide be placed over a fire in a glass capsule it changes in a short time, from white to yellow, and from yellow to red, like that exposed to light.

Easily and cheaply manufactured.

This oxide may be manufactured in quantity, and at little expense, so as to lessen the cost of certain preparations in which it may be employed. It may likewise be used as it is in painting.

Method of making it.

The following is the method I have employed to obtain this oxide. In a leaden barrel I enclose a certain quantity of small shot, with as much water as equals about one fifth of its capacity, leaving the rest full of air. This barrel is turned round by means of an axis fastened to each end. It is obvious, that it might easily be kept in continual motion by a stream of water.

To renew the air in the barrel, I introduce leaden tubes at various parts of its circumference, soldered to the sides so that no water can escape, and reaching internally to the axis of the barrel, while the extremities are a few inches above its surface*.

* This must make an unnecessary addition to the weight of the machine, and cost of materials, at the same time that they must be liable to injury. They would admit the outer air equally well, if the external aperture were level with the surface of the barrel. T.

IX.

An Essay on Instinct, read to the French National Institute, by Mr. DUPONT DE NEMOURS *.

THOUGH Descartes would have brutes to be mere machines, it is now the general opinion, that they are conscious of their sensations, and that their actions are determined by feelings of pleasure and pain; that they have a good memory; that from repeated experience they form general notions, founded on a sentiment of analogy; that they are guided by the pleasure or pain, which they are thus enabled to foresee, and this frequently in spite of the actual impulse of present pleasure or pain; and finally that these means, well managed, may be employed by man to educate them, and lead them sometimes to acquire a habit of executing with wonderful precision very difficult actions, and even some to which their structure seems not adapted.

Animals possessed of consciousness, and act upon it.

Neither does any philosopher doubt, that animals have various modes of expressing their wants and passions; and that those of a superior order, or which approach us in their organization, learn the signification of several of our words, which they obey without mistake.

Are capable of expressing their wants and passions.

But independent of these faculties, which resemble ours except in degree, and in which the different classes of animals differ from each other as much as some of them from us, naturalists have imagined they discern in certain species other faculties, which appear to them essentially different, and to which they have given the name of instinct.

Supposed likewise to have instinctive faculties.

These are certain actions necessary to the preservation of the species, but frequently altogether foreign to the apparent wants of the individuals, and often very complex; which we cannot attribute to reason, without granting them a degree of foresight and of knowledge, that every one would hesitate to admit. Neither can they be attributed to imitation; since it appears impossible, that the individuals by which they are practised, can have thus learned them, and yet those of the same species constantly practice them nearly in the same manner. And it is no less remarkable, that the

Some of their actions difficult to ascribe to reason, or imitation.

These most re-

* Magazin Encyclopedique, February, 1807, p. 437.

markable in animals apparently least intelligent.

actions which bear no relation to the degree of ordinary understanding are more singular, more intelligent, and more disinterested, in proportion as the animals that execute them belong to classes of a lower order and in every thing else more stupid. It is among the insects, molluscæ, and worms, that we observe the most admirable instincts. It seems as if instinct and reason were two faculties made to compensate and supply the want of each other; as on other occasions fecundity compensates the want of strength or long life. It is even by a due balance of reason, instinct, and physical qualities, as acuteness of the senses or bodily strength, that the species are continued.

Balance of reason and instinct.

These actions ascribed to an internal impulse.

Naturalists have imagined therefore, that animals endued with instinct perform their peculiar actions by virtue of an internal impulse, independent of experience, foresight, education, and all exterior agents; in other words, that it is their organization, which of itself determines them to act thus. This conclusion has been adopted by almost all observers: and if they have differed, it is only in explaining the manner, in which the organization can impart this determination. The following is one of these hypotheses.

Hypothesis.

The want or desire of a certain action can be occasioned only by sensations, or remembrances of sensations; in a word by images. It is not necessary, however, that a sensation should arise from without, for every external sensation requires interior movements of the brain and nerves, without which it would not have taken place: but these interior movements may originate in the organs themselves, without any external action, as is frequently the case in reverie, and in various diseases; nothing therefore prevents certain animals from being so organized, that internal movements shall regularly arise in them capable of producing certain sensations or images, and that these images shall imperiously determine their will to certain actions.

This does not lead to the doctrine of innate ideas,

This hypothesis appears to have nothing in common with that of innate ideas, the object of which is only general or abstract ideas: for they, who justly deny, that the general ideas of man are innate, have never pretended to assert, that man cannot have sensations from interior movements of his own organs, and without the intervention of external bodies; an assertion, that daily experience would have refuted.

Neither

Neither has it any thing in common with materialism; for, whatever idea we entertain of the intimate nature of the sentient principle, we are obliged to confess, that it experiences sensations only through the medium of the brain and nerves. materialism,

Finally, neither is it more closely allied than any other to fatalism: for, every action being determined, either by a present sensation, or by the recollection of a past sensation, or by the hope or fear of a future sensation, whether these sensations be external or internal does not alter the state of the question. or fatalism.

Mr. Dupont however appears to have been induced, to reject every sort of instinct indiscriminately, chiefly by the fear of splitting against one of these rocks. Mr. Dupont rejects instinct.

He begins by showing, that the actions of animals of the higher orders, as quadrupeds and birds, result from a combination of experience with their corporal faculties. In this there is no difficulty, as it is a point on which all naturalists are agreed. He then endeavours to explain physically how these animals, and children themselves, learn to suck. He shows, that several species are capable of uttering sounds sufficiently numerous to form a very complicated language; and he asserts, that he has observed them employ some of these sounds under circumstances so similar, as to leave scarcely any doubt of their attaching to them a fixed signification. His observations on this head are very interesting. His system.

He likewise endeavours to prove, that various species are capable of improving their operations under certain circumstances: though perhaps the naturalist will object to him, that he has sometimes taken different species for the same species improved. Thus the architect beaver of Canada is not precisely the same as the burrowing beaver of the Rhine; and the social spider of Paraguay is not the same with our solitary spiders. Animals capable of improvement.

It may be supposed, that the greatest difficulty Mr. Dupont has to encounter is in explaining, how insects have learned those wonderful precautions, with which they provide a shelter and proper nourishment for the egg, which they and sometimes even others are about to lay, and the maggot, that is to be produced from it; though frequently Difficulty in the case of insects providing for their young.

these

these insects have never seen, and never will see again, the egg, or a similar maggot; and the wants of the maggot have not the least resemblance to those of the insect that labours for it.

Curious instance of the sphex.

Among thousands of instances, that might be adduced, Mr. Dupont has chosen but one, that of the sphex, or ichneumon wasp. In this he cannot be accused of having taken an easy example. The following is its economy. During its existence as a perfect insect, it lives entirely on flowers. When it is ready to lay, it forms a cylindrical hole in clayey sand, and deposits an egg at the bottom of it. It then seeks on cabbages a small green caterpillar, on which it never preyed before; pierces it with its sting, so as to weaken it to such a degree, that it may be unable to resist the maggot, which is afterward to issue from the egg and feed upon it, yet not so as to kill it, that it may not putrefy; rolls it up in a circle; and lays it in the hole upon the egg. It successively proceeds in quest of eleven more of these, which it treats in a similar manner. It then closes the hole, and dies. The little maggot is hatched, devours the twelve caterpillars in succession, and changes to a chrysalis in the hole. As soon as its final metamorphosis is completed, it issues from its subterranean abode a winged insect, to enjoy itself among the flowers, till it is ready to lay, when it repeats the operations its mother had performed before it, and with caterpillars of exactly the same kind.

How explained by the author.

Mr. Dupont supposes in his explanation, that the perfect insect retains the remembrance of the sensations it experienced in the state of a maggot, though its form and organs are totally changed. He must likewise suppose, though he does not expressly say it, that the sphex can afterward distinguish by the sight* the caterpillar, and the sand, of which it acquired a knowledge only by feeling, and this by its ancient feeling of a maggot; for the maggot is blind, it lives under ground, and when it there becomes a winged insect the caterpillars are devoured. Lastly, Mr. Dupont dares

* This is not necessary: it may distinguish them by the smell, or in some other way; for it by no means follows, that, because man has only five senses, an insect has no more. W. N.

not

not admit, that the sphex foresees the egg it lays will produce a maggot, and will have need of all it provides for it: according to him it does this merely for amusement, in imitating what it perceived in its infancy.

X.

Observations on the Sulphurous Acid; by Mr. PLANCHE. *Read to the Society of Pharmacy, November the 15th, 1806* *.

MR. BERTHOLLET made known several remarkable properties of sulphurous acid, in two excellent Memoirs, read to the Academy of Sciences in 1782 and 1789. In the year 1796, Messrs. Fourcroy and Vauquelin read a much more extensive memoir on the same subject at the Institute, in which they gave a more complete history of this acid, and of its different combinations.

Changes produced by sulphurous acid on sirup of violets reddened by other acids.

I have considered with great attention the labours of these learned chemists, but among their numerous experiments I do not find any, which actually relate to the object of my present investigation; the changes that liquid or gaseous sulphurous acid occasions in sirup of violets reddened by different acids, and the contrary. This property of the sulphurous acid I am more eager to make known, as it may furnish matter for interesting reflections on the theory of acids in general.

The acid prepared by sulphuric acid and mercury.

The sulphurous acid I employed in my experiments was prepared by decomposing very pure sulphuric acid by means of mercury equally pure. In its preparation I followed the process of Berthollet. My sirup of violets was of a very fine blue, without any mixture.

Restored the blue that had been changed red.

Experiment 1. Sirup of violets, diluted with eight parts of distilled water, and coloured red by nitric, muriatic, sulphuric, phosphoric, or acetic acid, resumed its blue colour on the addition of liquid sulphurous acid. The colour was not quite so intense indeed, as before it was changed red, but it had no mixture of the latter colour.

* Annales de Chimie, Vol. LX. p. 254. December, 1806.

Exp.

Reddened again by the other acids.

Exp. 2. The acids above mentioned, added by little and little to the blue liquor, restored its former red colour immediately; the acetic acid excepted, the action of which was slower by a few minutes, and it required to be added in a pretty considerable quantity.

Exp. 3. Sirup of violets diluted with a similar quantity of water, and coloured red by oxalic, citric, tartarous, and acetous acids, had its blue colour equally restored by adding a few drops of liquid sulphurous acid: but on the subsequent addition of these acids they exhibited some peculiar properties, which I shall proceed to mention.

Oxalic acid.

1. The oxalic acid in a small dose produces at first no change. It must be added in considerable quantity, to make the liquor assume a violet hue; and several hours elapse, before it resumes its red colour.

Tartarous, citric, and acetous.

2. The tartarous, citric, and acetic acids, mixed in any proportion with the blue liquor, cannot again make it red, even though it remain exposed to the air for twelve hours.

The sulphurous acid continues to diminish the colour.

3. In these three experiments the blue colour continues to decrease; which indicates, that the sulphurous acid still enjoys its property of destroying colours, notwithstanding the excess of the other acids. All these experiments were made in glass vessels open to the air: but it was necessary to ascertain, whether this agent had any influence on the colour of the different mixtures; for which purpose I repeated the same experiments in bottles closely stopped, and operating as quickly as possible.

Experiments made in stopped Bottles.

Exclusion of air did not prevent the action of the sulphurous acid,

Exp. 4. Into nine flint glass bottles with stopples I put sirup of violets diluted with water as above, and reddened by the same acids, and ticketed them. Into each phial I dropped liquid sulphurous acid, till the blue colour was restored, taking care to shake the mixture well after each drop, and observe the change induced in its colour. This I did with all the nine phials in succession; and, stopping them as I did it, I left them at rest for six hours. In this space of time I observed the blue had lost a little of its intensity, without being affected with any tinge of red.

but modified the subsequent

Exp. 5. I had next to examine, whether the acids employed

ployed in the preceding experiments had equally the property here of reddening the sirup of violets, that had been rendered blue by the sulphurous acid, and the following were the results. action of the others.

With the nitric, muriatic, sulphuric, and phosphoric acids, the blue liquor changed to a vinous red:

With the acetic, to a light violet:

With the oxalic, to a pale rose colour:

With the tartarous, citric, and acetous, mixed in a very large proportion, there was no tint of red, but a remarkable diminution of the intensity of the blue.

Experiments with Sulphurous Acid Gas.

Exp. 6. It is well known, that the sulphurous acid in the state of gas acts with much more energy than in the liquid state. Accordingly I was desirous of examining its action on sirup of violets, diluted as before, and changed red by the same acids. I disposed my apparatus exactly in the same manner as for preparing sulphurous acid. As soon as the second phial, three parts filled with distilled water, was saturated, I opened a communication between it and a third, filled with a mixture of water and sirup of violets reddened by sulphuric acid. A few bubbles of the sulphurous acid gas were sufficient to restore the blue colour of the liquor. To this I substituted another phial, filled with a similar mixture, except that it had been reddened by a different acid: and thus I continued, till mixtures reddened by all the acids mentioned in the first experiment had been subjected to the action of the gas. I did not observe any very sensible difference between them; but it appeared to me, that the colour was less weakened by the sulphurous acid gas, than by the liquid sulphurous acid. Experiments with the gas. It did not appear to act more powerfully.

The slight difference, however, may have depended on the greater quantity of coloured liquor in the latter experiments, and the facility with which the effects of the gas could be observed, and its action governed.

These experiments repeated with sulphurous acid obtained by the medium of charcoal, or that of sugar, afforded similar results. The acid prepared in different ways acted the same.

SCIENTIFIC

SCIENTIFIC NEWS.

French National Institute.

Prize questions. Phosphorescent substances.

THE following prize questions are proposed for the year 1809. A considerable number of substances, under different circumstances, diffuse a phosphorescent light, more or less vivid, and more or less durable. Such are the fluate of lime, and some varieties of phosphate of lime, when thrown in powder on a heated body; the Bolognian phosphorus, when, after having been exposed to light, it is carried into a dark place; certain sulphurets of zinc, when rubbed with a hard substance, or even with a quill; rotten wood, certain fishes, and other animal substances approaching to putrefaction, when in the dark; &c. The Class of Mathematical and Physical Sciences therefore proposes as the subject of the physical prize, which it will adjudge in the public meeting of the first Monday in January, 1809, the following question.

"To ascertain by experiment what relations subsist between the different modes of phosphorescence, and to what cause every kind of it is owing, excluding from the examination the phenomena of this class that are observed in living animals."

The prize will be a gold medal of the value of 3000 fr. (125*l.*); and the papers must be delivered at the secretary's office before the 1st of October, 1808.

The term of the following question is prolonged from the 21st of March to the 1st of October, 1807, in consequence of the change made in the period of the annual meetings, which will prevent a decision on the papers from taking place before January.

Hibernation of animals.

"To determine by anatomical and chemical observations and experiments, what are the phenomena of the torpidity, that certain animals, such as marmots, dormice, &c., experience during winter, with respect to the circulation of the blood, respiration, and irritability; and to investigate the causes of this sleep, and why it is peculiar to those animals."

Messrs.

New members of the French Institute.

Messrs. Bosc, Silvestre, and Palisot de Beauvois have been elected members of the institute. Mr. de Beauvois, who succeeds Mr. Adanson, merited his success by his travels in Africa and America, the fruits of which were the Floras of Owerra and Benin, already published, and that of the United States, which he is preparing for the press, as well as by researches concerning the cryptogamia class. These researches have not only furnished descriptions of new species and genera, but more particularly a system of the fecundation of mosses and mushrooms, of which we shall give a brief outline.

Beauvois's system of the fructification of mosses,

Amid that dust of the capsules of mosses, which Hedwig considered as the seed, is a kind of nucleus, or little axis, more or less swelled, called by botanists the columella. In this nothing has been observed but a parenchyma, more or less cellular; and so it is represented repeatedly by Hedwig. In this Mr. de Beauvois says he has perceived very small grains, which he believes to be the true seeds; and the other dust, that fills the capsule around it, he supposes to be the pollen. When the capsule is ciliated, the setæ by their motion compress the pollen against the seeds, to fecundate them, at the moment when they are about to escape.

and of mushrooms.

With respect to mushrooms his opinion is similar. The multitude of little grains, or dust, spread over the gills, or other parts of some, and included in others, as the lycoperdons, which have been supposed to be seeds, are according to him the pollen; which in the same manner fecundates the true seeds, that are contained within the gills, or part covered with this pollen, just as they burst from these.

New name of the class.

In consequence of this opinion, Mr. de Beauvois has taken the liberty of substituting the term ætheogamia, or uncommon fructification, to that of cryptogamia, to which the class is equally entitled, even on his own hypothesis, and which is certainly more scientific.

Prodromus of the class Ætheogamia.

Part of his *Prodrome d'Æthéogamie* is published, in which he has announced his distribution of the mosses. In this he has some claim to impartiality; for while in forming his genera he rejects the sexual organs of Hedwig, he takes no account of the columella, which he considers as the pistil. In the second part, which is about to appear, he has reduced

duced the number of genera of the mushrooms to sixty, which he distributes into six orders.

Seeds of the parasitic fungi pass through the epidermis of plants.

In a subsequent essay he asserts, that he has seen on young plants particles appearing to him similar to the seeds of parasitic funguses, that are accustomed to unfold themselves in the substance of the plants, underneath the epidermis: and hence he concludes, in opposition to Mr. Candolle, that these grains pass through the epidermis, to lodge themselves beneath it.

Mushrooms increasing by horizontal layers.

He treats more largely on certain mushrooms, that grow by layers from the top downwards, contrary to other vegetables. This observation is not new; but his opinion is; for he considers each layer as a new mushroom, produced from the seed of the layer above it.

The raphia of Owerra different from the sago tree.

He has likewise shown, that the flowers of the raphia of Owerra differ too widely from those of the sago tree of the Moluccas, to continue them in the same genus of palms.

Mr. de Candolle.

An unsuccessful competitor of Mr. de Beauvois was Mr. de Candolle, who, though young, has distinguished himself in vegetable physics, as well as in other branches of botany. Among his labours may be particularly noticed his observations on the action of artificial light, which, operating at first imperceptibly, at length effects a total change in the habits of vegetables: on the cortical pores: on the production of oxigen gas by green lichens, which has been denied, but the reality of which he has proved: and on the vegetation of mistletoe, which really attracts the sap of the apple-tree, but cannot draw up water, in which it is directly immersed; a fact of importance with respect to the cause of the ascent of the sap in plants.

Action of artificial light on plants.

Production of oxigen by lichens.

The mistletoe attracts sap, but not water.

Parasitic funguses.

Mr. Candolle presented three memoirs to the class on the occasion. The first was on those parasitic funguses, that develop themselves beneath the epidermis of plants, and cause several fatal diseases, as the blight in corn. (See Journ. vol. X. p. 225.) It has been supposed, that the seeds of this plant were introduced through the pores of the epidermis: but as coloured liquids traverse these pores with difficulty, and simple application does not inoculate the plants with these diseases, he conceives the seeds to be introduced by the roots with the nutritious juices, and circu-

Their seeds introduced into plants by the roots.

late

te with them till they arrive at places suitable for their velopement. He compares them in this respect to intes-nal worms, which can subsist only within the bodies of her animals. From this theory, and the observation, that ch parasitic fungus is capable of being propagated only plants of the same family, he deduces rules of which the rmer may avail himself to avoid the contagion. Eighty-ur species of these fungi were already known, and Mr. ndolle has added more than a hundred to the number.

Near 200 species.

In a memoir on algæ he has shown, that these marine ants have no true roots; that there is no trace of vessels their organization; that their whole surface absorbs moisre; and that the greener they are the more oxigen gas is tricated from them by light He adds, that the little ains, hitherto considered as their seeds, are merely caples, and contain seeds much smaller, enveloped with a scous matter, which fixes them where they are to grow.

Algæ.

Another unsuccessful competitor was Mr. du Petit-houars, who resided a long time in the isles of France and ourbon, and visited Madagascar. He has begun to pubsh a Flora of these places, rich in singular plants. His bservations on the germination of the *cycas*, or sago tree, hich some have considered as a palm, others as a fern, ave convinced him, that it ought to constitute a separate mily, equally distinct from both.

Du Petit-Thouars.

The sago tree distinct both from the palms and the ferns.

Mr. Ventenat has published the 20th number of his Garen of Malmaison, but ill health has obliged him to take me respite from his labours.

In Mr. de la Billardiere's 23d number of his Flora of ew Holland, he describes a tree by the name atherosperma, hich he considers as belonging to the family of ranunculi, at may probably become useful in France. Its nuts have he taste and smell of nutmegs, and it appears capable of nduring the climate very well.

A fruit resembling the nutmeg capable of growing in France.

Mr. von Humboldt, and his fellow traveller, Mr. Bonland, continue the publication of the plants they observed South America. The genus melastoma alone furnished hem with so many new species, that they might have filled separate work with them.

Von Humboldt.

They

The condor.

They have not less enriched the science of zoology. The condor has never before been so accurately described. Its size has been much exaggerated. It scarcely exceeds a metre (3 feet 3 in.) in height, or three or four in spread of wing. Its general colour is blackish brown; and round the lower part of the neck is a collar of white feathers. The male is distinguished by a fleshy crest on the top of the head, and a white spot in the wing.

Electrical eel of Surinam.

They likewise made some curious observations on the gymnotus electricus. In the water it is capable of giving such a shock to a horse, as to stun it, so that it falls down, and is in danger of being drowned. Mr. von Humboldt, putting both his feet on one just taken out of the water, felt an acute pain, that did not entirely go off the whole day. Slighter shocks induce a peculiar trembling, a kind of twitching of the tendons, different from those of common electricity. The pain is more like that produced by galvanizing a wound.

Mr. Tenon has given an important continuation of his Memoirs on the Dentition of the Horse.

Fossil remains of lost animals.

Mr. Cuvier continues his inquiries concerning the animals, that appear to have been destroyed by some revolutions of the globe. He has described five in the last half year, all of the genus mastodontes: the characters of which are to have tusks and a proboscis, and their grinders furnished with conical protuberances arranged in pairs. In the plaster quarries of Montmartre a skeleton of one of the species described by Mr. Cuvier has lately been dug up nearly entire.

Genus mastodontes.

Beauvois's insects.

Mr. de Beauvois has published the third number of his insects collected in Africa and America.

Iron rendered hot and cold short by chrome, phosphorus, and manganese.

Mr. Vauquelin has instituted an accurate analysis of the iron ores of France, their products, the fluxes employed, and the scoriæ, with a view to ascertain the causes of the defective qualities of the iron. These he attributes to remains of chrome, phosphorus, and manganese. He observes too, that this compound, sublimed in the furnaces, bears much resemblance to that of the stones that have fallen from the atmosphere, except that these contain nickel also; and

and he conceives it not impossible, that the particles carried up from our furnaces may contribute in some degree to their formation.

Sparry iron ores.

Messrs. Descotils and Hassenfratz too have been examining the sparry iron ores; and the former ascribes the infusibility of some of them to magnesia, which the latter denies. Mr. Lelièvre has described a mineral, that has been hitherto confounded with the iron spars, which he finds to consist of more than half oxide of manganese, near one third carbonic acid, only eight per cent of iron, and two and half per cent of lime. He has likewise described a stone, which he found in the island of Elba. This contains more than half oxide of iron, a little oxide of manganese, and the rest is silex and lime. Its crystalline nucleus is a prism with a rhombic base, its colour black and opake, its hardness a little inferior to that of feldtspar, its specific gravity 4. Mr. L. has named it *yénite*, from one of the most memorable events of this century. [From the battle of Jena we presume; a mode of composing new names, in which we trust he will be followed by few of the real friends of science.]

A carbonate of manganese confounded with them.

Yenite.

Ancient pewter.

Mr. Baraillon having discovered some ancient pewter vessels in digging among the ruins of the Roman town of Neris, near Montluçon, they were analysed by Mr. Anfrye, inspector general of assays at the mint, and found to contain thirty-five per cent of lead.

Method of freeing alum from iron.

To the different modes of freeing alum from iron Mr. Seguin has added another, founded on its difference of solubility when contaminated with iron, and when pure. By dissolving sixteen parts of common alum in twenty-four of water, and crystallizing, he obtains fourteen parts of alum as pure as the Roman, and two nearly the same with that of Liege. This process might be adopted in the first instance in manufacturing alum, so as to enhance its value one third.

Heat a vibratory motion.

It is known, that count Rumford adheres to the old theory of heat being simply a vibratory motion of the particles of bodies. As a strong objection to this has been adduced the production of heat by condensation, as if some sub-

stance were mechanically pressed out of the pores of bodies thus diminished in bulk. In answer to this he has shown, that some cases of condensation are accompanied by the production of cold. Thus solutions of several salts, being mixed with pure water, lose at the same time both bulk and heat. The generation of cold by dissolving salts is a well-known phenomenon, and has been ascribed to the necessity of a solid's absorbing heat when it is converted into a liquid: but here this explanation will not apply, as the solid is already dissolved, before it is mixed with the water.

Heating water by steam applied to soap-boiling.

Count Rumford has likewise made a very happy application of the process of heating water by steam to the manufacturing of soap. He has succeeded in boiling soap to a proper degree by its means in six hours, which in the common mode requires sixty. He conceives, that this saving of time is partly owing to the concussions given to the mixture of oil and lie by the heated vapour forced into it, and there suddenly condensed.

Improvement in boilers and evaporators.

He has also made a new improvement in boilers for heating or evaporating liquids. This consists in adding to their bottoms several tubes, which descend into the flame, so as to be surrounded by it on all sides; thus increasing the surface of the bottom, without adding to its diameter.

(In our next we shall give an account of the Transactions of the Mathematical Division of the Class.)

Correction.

The *Camera Lucida* described in our Journal, No. 71, p. 1, is sold not only by Mr. Newman, but also by Messrs. P. and G. Dollond, St. Paul's Church Yard.

Apparatus for triturating Quicksilver.

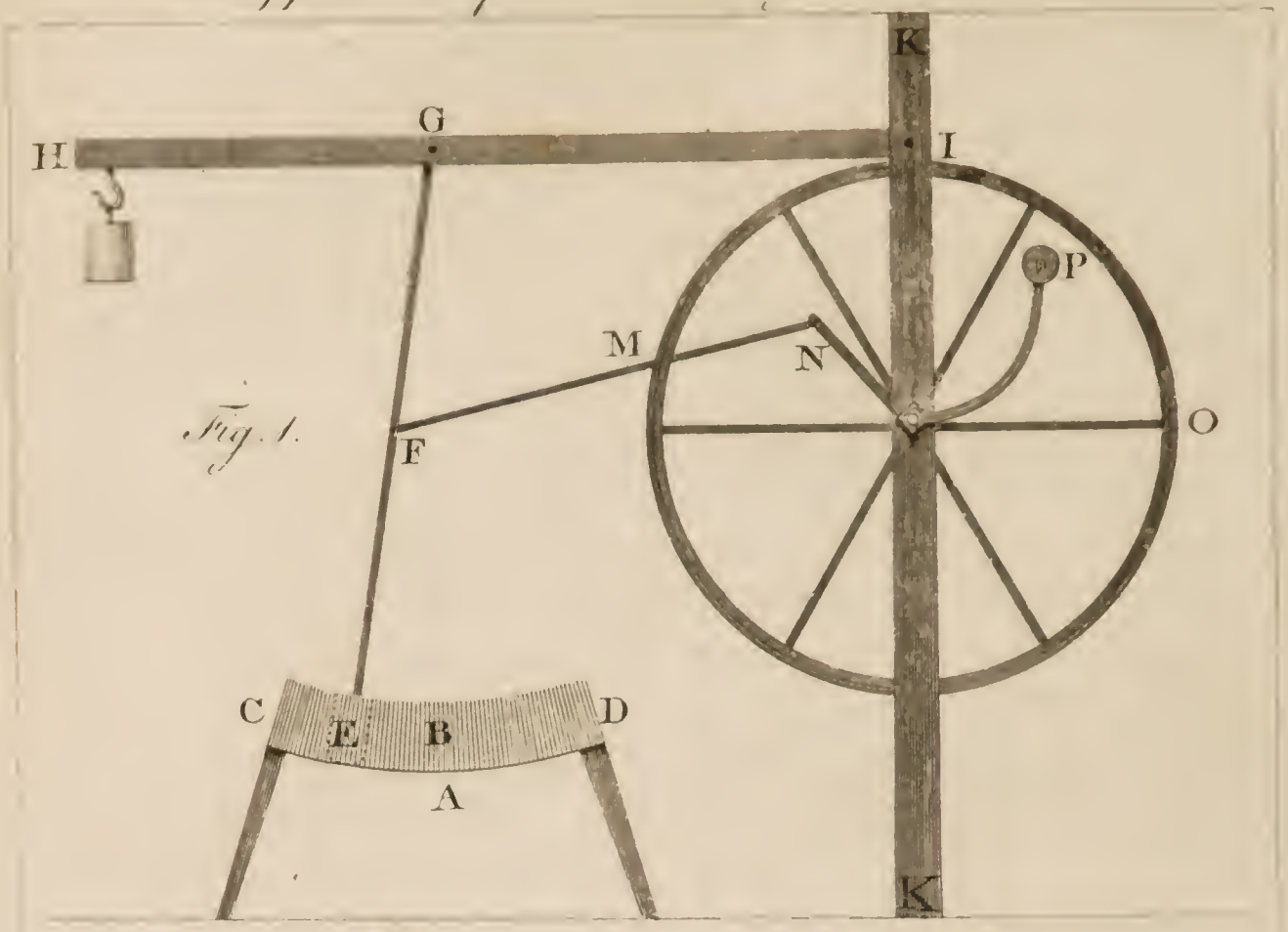


Fig. 1.

Fig. 2.

Fig. 4.

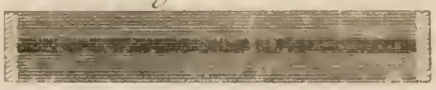
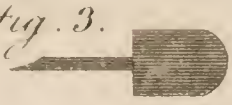


Fig. 3.



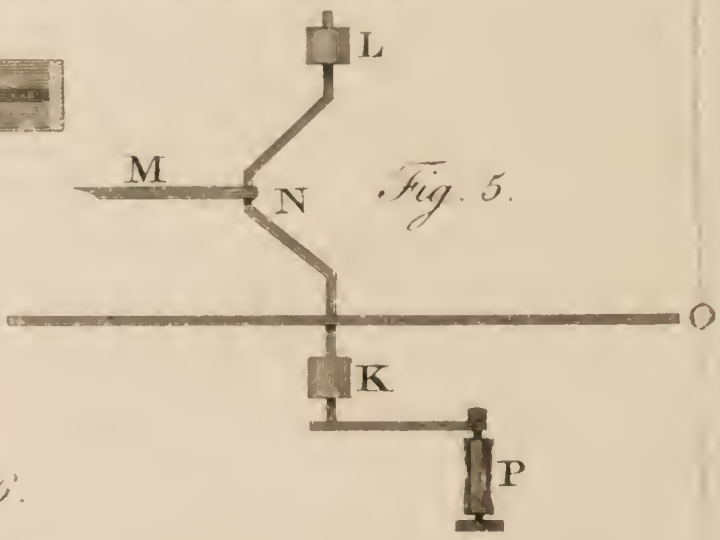
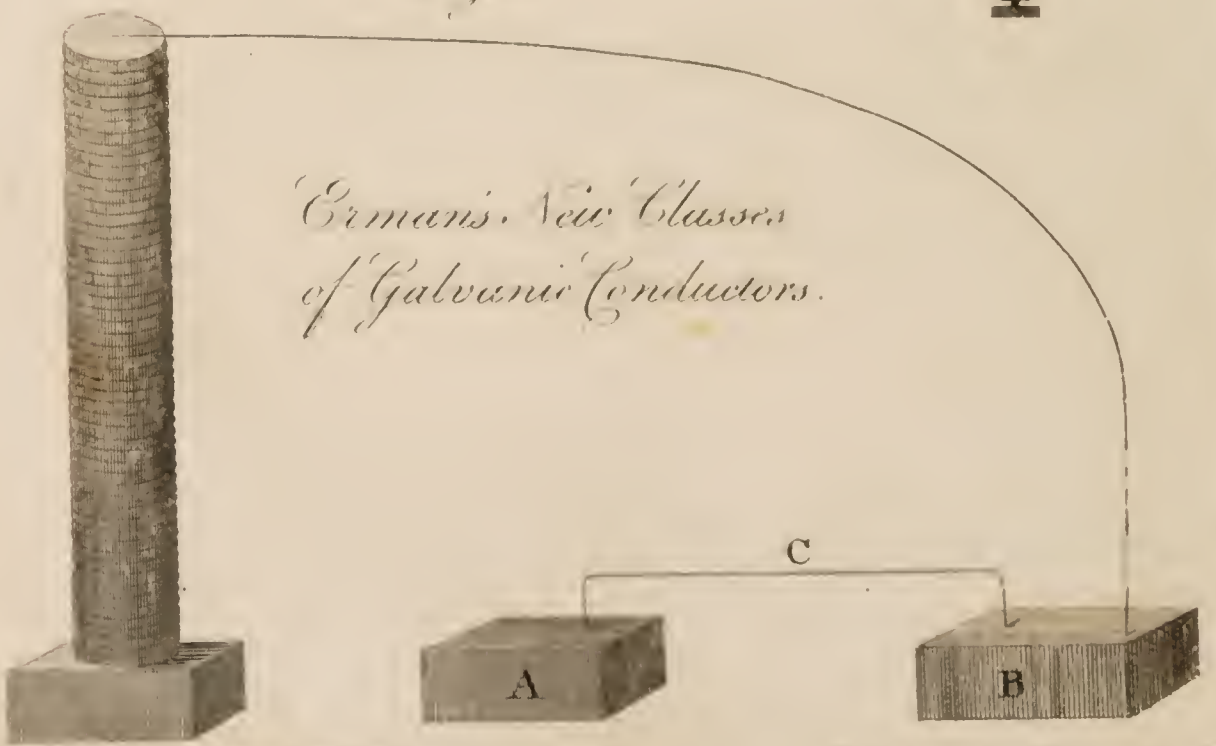


Fig. 5.

Fig. 6.



Erman's New Classes of Galvanic Conductors.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XVII.

ARTICLE I.

Description of a Machine for triturating and combining Quick-silver with other Substances, by a CORRESPONDENT.

SIR,

THE difficulty and tediousness of the process of combining pure mercury with mucilaginous or fat substances by trituration with a pestle and mortar, so as to bring it to that state of extreme division, in which alone it can exert all its efficacy as a medicine, are well known; on account of which some have recommended the use of a small quantity of flowers of sulphur, or of sulphuretted oil, others that of rancid fat, each of which operates by its chemical action on the mercury, and so far is at variance with the original intention. In forming mercurial plasters the use of some such substance as sulphuretted oil, or turpentine, has been found particularly necessary; and owing to this perhaps less benefit has been derived from them, than the practitioner has expected. I trust therefore an account of a machine, contrived to produce the effect very speedily and with little labour, may not be unacceptable to many of your readers.

Combination of mercury with fats or mucilages tedious.

Means of accelerating it injurious.

Hence mercurial plasters perhaps defective.

Machine for the purpose.

The apparatus consists of a piece of cast iron, A, Plate IX. Fig. 1. about two feet long and four inches wide, curved so as to form a segment of a circle of four feet radius.

Description of the apparatus.

Description of the apparatus.

Perpendicularly to each side of this segment is fixed an additional piece of iron, B, by screws or otherwise; and another piece at each end, C, D, inclining in the direction of the radius of the circle; so as to stand above it about four inches, and form a box or trough. This may be made to stand on legs, or be fixed securely in any simple framing, at a height most convenient for the person that works or attends the machine.

A wrought iron pallet, E, is to be fitted accurately into the box, reaching from one side to the other, allowing it only sufficient space to work easily backward and forward. The lower end of the pallet is to be made to fit the bottom of the box; but its lower edges must be rounded off considerably, so as to rise over the matter in the box, and not drive it all before it. Its shape is more particularly seen at Figs. 2 and 3, which are on a somewhat larger scale, Fig. 2 being the front, and Fig 3 the side view of it.

This pallet is to be affixed to the end of a vertical shaft or rod, F, measuring four feet from the extremity of the pallet to the pivot, G, on which it turns. The top of the rod may be secured by working through a chaff mortice, which will allow it to move backward and forward, but not admit any lateral motion.

The piece, H I, in which this mortice is made, is fixed to a cross piece between the uprights, K, L, Fig. 5; one of which only, K K, is seen in this view: and as this cross-piece moves on a pivot at each end, though it is prevented from moving horizontally, it is confined vertically only by a weight at the extremity; which weight may be greater or less, according to the degree of pressure or friction required. Instead of the weight, a wooden or other spring might be made to act on the head of the shaft at G; but in general a weight will be found preferable.

The pallet is set in motion by means of a rod M; one extremity of which is attached to the vertical shaft at F, where it works on a pin; the other by coupling brasses to the crank, N, in the axis of a fly wheel, O. A perpendicular view of these parts, with the same letters of reference, is given at Fig. 5. The place where the rod, M, is attached to the vertical shaft, F, must be so proportioned

to

to the throw of the crank, that at every revolution of the wheel the pallet shall move backward and forward through the whole extent of the box; to which a cover may be fitted, with a longitudinal aperture sufficient for the shaft, as shown at Fig. 4.

Mode in which it acts.

The mercury, and the composition with which it is intended to be mixed, being placed in the box or trough, half on one side of the pallet standing in the middle, and half on the other, the fly wheel is to be turned by its handle, P, as in common operations. As long as the mercury remains in a fluid state, by its gravity it will follow the pallet to the centre of the box; and as some portion will mix with the composition at every turn, the whole will soon be completely blended together.

Particularly adapted to mercurial plasters.

This apparatus is particularly adapted for combining mercury with a composition of sufficient tenacity to form a plaster, which cannot be done directly in the common way; so that it is necessary first to subdue the quicksilver with turpentine, or sulphuretted oil, and then to mix it with a plaster previously melted. But with this apparatus the plaster is softened by the heat generated by the friction, and the power is sufficient to mix the mercury with it directly; and this both intimately and speedily.

Applicable to various other purposes.

It is almost superfluous to say, that the apparatus need by no means be confined to the dimensions here given; and by enlarging them it may be adapted to various useful purposes, which will readily suggest themselves to you. Where this is done, however, it might be found necessary to steady the horizontal piece, H, I, by allowing its extremity H to work between two uprights.

I am,

SIR,

Your obedient humble Servant,

W. X.

July 20, 1807.

II.

A Memoir on Two new Classes of Galvanic Conductors, by Mr. Erman.

(*Concluded from p.* 249.)

Sect. II. *Of Conductors, that, in establishing a Contact between the Two Poles, insulate the positive Effect, while they continue to propagate the negative Electricity*

Substances that act as conductors to either pole separately, but only to the negative, when they are in conjunction.

I HAVE placed in a fifth class those substances, which, applied to either pole separately, act as excellent conductors, but which, interposed between both poles, insulate the positive effect, without discontinuing to be perfect conductors of the negative. A wish to realise all the combinations possible in closing the galvanic circle, excited me to examine a great number of substances, in order to find some one that should come under this description. My attempts were long in vain, because the analogies that guided my research were very imperfect; and I did not discover the property in question in the flame of phosphorus, before I had found it unquestionably to exist in a solid body. This body is alkaline soap of every kind, provided it be in the highest state of dryness possible: at least I have found no perceptible difference in the electric effects, whether it were composed of vegetable oil or animal fat, converted into soap by pure soda, or soda mixed with potash, and prepared for pharmaceutical purposes, or the uses of the arts and domestic economy. All these soaps exhibited the effects I am about to describe, provided the essential condition of their being desiccated as much as possible were fulfilled.

Flame of phosphorus and soap.

Hard soap, perfectly dry, applied to either pole, is a conductor.

A prism of hard soap, completely dried, and applied to either of the poles of a galvanic pile, conducts all the electricity of that pole into the ground, and produces a maximum of electric intensity at the opposite pole. In this respect there is no difference between the two poles, and the soap acts as the most perfect conductor would do. Of this I convinced myself by measuring with Volta's electrometer the divergence produced at each of the poles by the contact

of

of metal, a wet finger, soap wetted at the point of contact, and soap perfectly dry; and I found them all equal in degree. It will soon appear why, notwithstanding this, no shock is obtained on employing a prism of soap, unless it be wetted at the point of contact.

If a wire from each pole communicate with an insulated piece of soap, the circuit will not be completed.

If now two wires, issuing from the two poles of the pile, have their extremities fixed in a perfectly insulated prism of soap, into which they should penetrate a few lines, no remarkable effect is perceived: that is to say, after having brought the two poles to the same intensity, by applying to them an insulated metallic rod, the electrometers of the two poles will act as they did before the intervention of the soap, and when a stratum of air insulated them perfectly with respect to each other.

If this soap be uninsulated, the negative electricity will be conducted off, but not the positive.

But the instant a free communication is established between the soap and the ground, the positive electrometer exhibits a maximum of divergence, and that of the negative side loses all signs of it, precisely as if a communication had been established between the ground and the negative pole itself. Consequently the soap, which insulates the positive effect, is a perfect conductor for the negative; to which it belongs throughout its whole extent, for if you touch the soap with a fine point ever so near the place into which the positive wire is inserted, it is impossible to take from it any portion of electricity, so perfect is the insulation of this pole.

Touching the positive wire with a dry finger and the soap with wet does not form a communication: if both fingers be wet a shock is felt.

A very striking proof of this paradoxical property is, if one finger be applied to the wire of the positive pole, and another finger wetted to the soap, no shock is felt, and the electrometers do not show the least change in their respective divergencies. But if the experiment be repeated by establishing a communication between the positive pole and the soap with both fingers wetted, a very perceptible shock will be felt, and the two electrometers will arrive at an equal and a very weak degree of intensity.

These facts are sufficient to establish the existence of this fifth class of substances; but on pursuing our researches farther we meet with many interesting phenomena.

An apparatus for decomposing water forming part of the positive

To discern these the better, the continuity of one of the wires should be interrupted, and an apparatus for extricating gasses be interposed between its parts. In this case no

hemical

chain is not affected, even if a wet conductor be in contact with the soap and negative wire.

But the circle is completed by the least wet between the soap and positive wire.

chemical effect will be perceived, the insulation of the positive wire being an insurmountable obstacle to it. If now a little sponge, or a piece of cloth, be wetted with water, and placed in contact both with the negative wire and the soap, every thing will remain as before, and there will be no trace of chemical decomposition. But the moment this wet conductor is so placed, as to touch at the same time the positive wire and the soap, gas will be extricated in torrents, and the electrometers will indicate the completion of the galvanic circle. Thus the smallest quantity of water is sufficient, to destroy at once the anomaly of insulation, which characterizes this substance, and convert it wholly into an excellent conductor. I have frequently seen this effect result from the simple application of a piece of money, which I had damped on one side merely by breathing on it, and which I afterward placed on the surface of the soap, and in contact with the positive wire; while the same piece of metal, in the very same position, produced no effect in its usual state of dryness.

A coin damped on one side.

No fluid but water produces this effect;

I know no fact, where the indispensable necessity of water in a galvanic action declares itself in a more astonishing way: for the property of converting the whole mass of soap into a perfect conductor for the two poles in communication, by the contact of the positive wire, belongs exclusively to water, and is not, as might perhaps be supposed, a property of fluidity in general. Mercury, naphtha, oils of every kind, and other liquors not aqueous, poured into a hollow made in the soap at the spot where the positive wire is inserted, produce not the least effect.

and it is decomposed in the process:

It is very remarkable too, that water thus applied between the positive wire and the soap undergoes the same chemical decomposition as in the apparatus for decomposing it. In fact, according to the nature of the metallic wire, with which the water or wet conductor is in contact, either an oxide will be produced in abundance, or a gas, which is easily discernible by the froth it occasions. Hence it is, that the time during which the interposed water produces its effect is always limited, being in the direct ratio of the quantity employed, and the inverse ratio of the intensity of the pile: but in all cases both the electrometrical and

so that the effect ceases when all the water is decomposed.

chemical

chemical effects, which depend on the presence of water, continue decreasing, and soon cease entirely, when all the water at the points of contact is decomposed. From that moment the soap resumes its characteristic property, and insulates the positive electricity.

It will be proper to introduce here an observation of some importance to the success of experiments of this kind. They who would repeat them without being able to procure prisms of soap exposed to the air for some years, or completely dried by the action of an oven or a stove cautiously conducted, might be tempted to accuse me at first of not having seen clearly; for a communication being established between the polar wires by soap yet damp, both the electrometers and the apparatus for decomposing water will begin by indicating a more or less perfect completion of the galvanic circle. But the part that water acts in these phenomena perfectly explains this want of success. It is the portion of free water, interposed in the damp soap between it and the positive wire, that in this case conceals the characteristic property, by which this substance belongs to the fifth class. To evince this nothing more is necessary, than to suffer a few moments to elapse: the water foreign to the conditions of the experiment will be consumed with more or less rapidity, according to its abundance and the energy of the pile; and then the whole of the soap will insulate the positive electricity, while it will serve as a conductor to the negative. On taking out the positive wire, that has thus been inserted into damp soap, the point will be found oxided, if the metal be of a nature to admit it, which never takes place in soap perfectly dry. It is scarcely necessary to add, that, if this wire be cleaned, and inserted into any other part of the damp soap, the same exception to the general rule will again recur, since in this new point of contact the conductor will find a fresh portion of free water. The oxidation of the positive wire in damp soap might lead to the supposition, that the insulation of the positive pole is owing to the production of this non-conducting coat. But the contrary may be proved by employing platina wires, which exhibit the phenomena in question, as soon as the water interposed by chance or design

If there be any moisture in the soap therefore, the operator will be deceived at first, the soap acting as a perfect conductor.

But this effect ceases by waiting a little, till the water in contact with the wire is decomposed.

Though the soap will act as a conductor again, if the wire be inserted in a fresh part.

The cessation of the effect is not owing to oxidation of the wire.

design is consumed on the positive side by the chemical action of the pile, without exhibiting the slightest trace of oxidation. Besides, the wires most easily oxided show no appearance of oxidation, when they have been employed to establish a communication between the positive pole and a prism of soap perfectly dry.

The water must be in contact with the positive wire.

An experiment of importance on other accounts shows, that the water must be applied to the precise point where the positive wire touches the soap, in order that the positive effect may be propagated as well as the negative. Let A B, Plate IX. Fig. 6. be two prisms of soap perfectly dry. Into each introduce one of the polar wires of the pile, and then connect them together by a wire C, forming an arc from one to the other. The electrometers of the pile will indicate a complete insulation of the positive pole; the contact with water of either of the two prisms, or of the intermediary arc C, will constantly discharge the negative electrometer, and carry the positive to a maximum of divergence. If now a wet conductor be applied between the wire of the positive pole, and the prism A, into which it is inserted, the electrometer will indicate, that the prism, and likewise the whole of the intermediate arc C, belong to the positive pole; since on touching these parts of the apparatus the electrometer of the negative pole is made to diverge, and the positive side is discharged. But the prism B belongs wholly to the negative pole, and on touching it divergences are produced the reverse of those that occur on touching A. The circle therefore is not completed: and in fact, if an apparatus for decomposing water be interposed, no chemical effect takes place; while on touching the two prisms at the same time a shock is felt, if the fingers have been wetted, and the pile has a certain degree of energy. In all cases the simultaneous contact of the two prisms excites in a prepared frog very strong contractions. But all these effects, which depend on the insulation of the positive pole, cease, and instead of them gas is produced in the interposed apparatus, the moment a second wet conductor is applied to the point where the intermediate arc C touches the prism B, because it is at this point, that the arc C exhibits the positive effect.

It

It is decidedly shown therefore by experiment, that the humidity of the whole mass of soap goes for nothing in these effects, and the precise point at which the water should be interposed is indicated with the greatest precision.

The humidity in the other parts of the mass of soap has no effect.

I cannot help inviting those, who strictly refer all the phenomena of the pile to a material and effective circulation of the electric fluid, maturely to weigh this experiment without prejudice; for in this way of explaining them the phenomena of the fifth class can arise only from a greater difficulty the fluid experiences, when it has to enter into the mass of soap, while its exit is infinitely more easy. But how comes it then, that the positive pole is so completely charged by touching the prism A, before the interposition of wet conductors? Here certainly the electricity of the ground must have entered into the prism B through the intermediate arc with the greatest facility. And why does it not enter in the same manner into the prism A by the positive pole?

Difficulty of accounting for the phenomenon by the circulation of a single fluid.

For my part I have not yet entirely renounced the hypothesis, that the efficacious cooperation of water in the physical and chemical effects of completing the galvanic circle is intimately connected with the property it has of dividing itself then into two zones, one of which exhibits electrical effects the reverse of the other. This mechanism of electric partition, this polarity of water and all humid conductors, announce themselves in such a striking manner, when we apply them to the soap, that I can scarcely believe the physical and chemical effects produced by completing the circle through the intervention of humid conductors are not owing to this very mechanism. Whatever may be the fate of this hypothesis, the developement of which would lead me too far from my subject, the following facts appear to me deserving of attention.

Water perhaps acts by separating into zones of opposite electricities.

The pile and prism of soap being perfectly insulated, let the wire of the negative pole be inserted into the soap; and let the other extremity of the prism be connected with the positive pole by means of a thoroughly wet hempen string six or seven inches long. It is obvious, that from this interposition of a wet conductor between the soap and the positive pole the galvanic circle must be completed;

If the connection between the soap and the positive pole be made by a wet string,

as in fact the electrometers and the apparatus for decomposing water show. If now two gold leaf electrometers be placed in contact with the two extreme portions of the wet conductor, we shall find, as long as the circle continues effectively completed, these two electrometers will exhibit opposite divergences; for that which is nearest the soap will diverge negatively, while that nearest the pile will diverge positively. This may be proved by touching that part of the string nearest the soap, by which the electrometer contiguous to it will be discharged, and the charge of the other electrometer will be considerably augmented; but the reverse will take place, if the part of the wet string nearest the pile be touched. The partition of electricity into two opposite zones therefore is beyond a doubt.

the end of the string next the soap will exhibit negative electricity, the other end positive.

Now let the positive side communicate with the soap by means of a wire, and let the wet conductor be interposed between the soap and the negative side, the circle will not be completed, and no chemical effect will take place, as has already been observed. But neither will the string exhibit any partition into electric zones: for if two electrometers be applied to the two opposite ends of this string, they will both diverge in the same direction, and in the same manner; and by touching the string in any part both will be deprived of their divergence. Now to destroy this homogeneousness of electrification, and communicate to the string the most decided polarity, it is sufficient to apply a wet conductor between the soap and the wire of the positive pole; for the moment its interposition has completed the circle, the two electrometers at the ends of the string will diverge in opposite directions; by discharging one the other will be charged; and this partition of opposite electricities will remain, as long as the apparatus for decomposing water continues to indicate, that the circle is complete. This completion of the circle and partition of the zones will cease at once, if the wet conductor applied to the wire of the positive pole be removed. I could wish, that other natural philosophers might be struck like me with the singularity of this accordance of effects, the importance of which I think I foresee, if it be farther pursued.

If the wet string be applied between the soap and the negative side this partition of electricities will not take place,

till a wet conductor has been placed between the soap and the positive wire:

and this effect will cease, as soon as the latter conductor is removed.

Among the numerous combinations I have tried, to as-

Soap will serve as a connecting

 certain

certain with some precision the particulars of the phenomena, that are afforded by soap applied to the galvanic pil, I have observed nothing, that is not completely explained by the definition of conductors of the fifth class. Thus a prism of soap applied to the positive and negative extremities of the two piles, each of which has the same number of plates but in an inverted order, connects these piles completely, as long as the object is not to complete the galvanic circle; and give to their poles the same divergences, as if they were connected by a perfect conductor. But to obtain the physiological or chemical effects, that require the completion of the galvanic circle, a wet conductor must be placed between the prism of soap and the positive pole. If this interposition were made at the negative pole, it would have no effect. The reason why I mention this experiment, which is only a corollary from what has been already said, is to point out a very direct solution of a point of theory, on which philosophers differ, that may be drawn from it.

medium between two piles, but not for completing the circuit,

without a wet conductor to the positive pole.

It has been asked, what kind of electricity belongs exclusively to each of the two different metals of the pile; and opinions have been divided on the point. They who assert, that the elements of the galvanic pile are silver, a wet conductor, and zinc, ascribe the positive electricity to the silver. They on the contrary who maintain, that the proper combination is silver, zinc, and a wet conductor, consider the zinc as the metal charged with positive electricity. I have hesitated some time between the two parties, for want of a direct unequivocal proof, and from unwillingness to sacrifice my scruples to the authority of Volta himself. Now it appears, that the properties of conductors of the fourth and fifth classes furnish the most direct and palpable means of deciding the question. Among several other analogous proofs, the following is one of the most evident, and most easy to be exhibited.

This leads to a solution of the question, whether the silver or the zinc be in the positive state.

Between the last pair of plates of zinc and silver in any pile, place, a slice of perfectly dry soap, then establish a communication between the two poles by the interposition of an apparatus for decomposing water, and no chemical effect will be produced. With a camel-hair pencil lightly moisten

A piece of dry soap being placed between the last two plates, no effect is produced:

if the side next the silver be wetted, it is the same: if the side next the zinc be wetted the pile acts effectually. The zinc therefore is in the positive state.

moisten that surface of the soap, which is in contact with the silver, and again establish a communication between the two poles: still it will be the same. But the moment that the soap is moistened on the side that touches the zinc, the chemical and physiological effects will exhibit themselves fully. Now as we have already found, that a wet conductor is efficacious between the soap and the positive side exclusively, it is demonstrated beyond controversy, that it is the zinc, and not the silver, which constitutes the positive agent in the pile.

I know not at present what other substances belong to our fifth class. It appeared above, that the flame of phosphorus must decidedly be referred to it. Frequently I have seen indications of the same property in animal jelly reduced to a certain degree of dryness, as well as in ivory: but other masses of these substances exhibited these phenomena in a very equivocal manner, so that I refrain from deciding upon them, and at present shall only mention soap and the flame of phosphorus as included in this class.

Animal jelly and ivory have sometimes appeared to belong to this fifth class.

Many supposed nonconductors might be examined.

It would be interesting to examine, with a view to this classification, a great number of substances, which have been considered as nonconductors, because the galvanic circuit is not completed by their interposition: but it is now completely proved, that this test is insufficient; and the argument in favour of the nonidentity of galvanism and electricity, taken from the mode of action of flame, shows that errours of this kind may prove dangerous to the theory.

Why is ice a nonconductor, water a bipolar conductor, and vapour again a nonconductor?

The field of observation here opened may prove fertile in general results for the chemistry of electricity. By what mechanism of action is it, that water, so far divested of caloric as to become solid, perfectly insulates the effects of galvanism, as I have elsewhere shown: that afterward impregnated with a certain quantity of caloric in the liquid state, it transmits these effects with certain modifications, dividing itself into two zones, one of which is a conductor of positive, the other of negative electricity: and that lastly this same water, in passing to the elastic state by an excess of caloric, returns again to the class of perfect nonconductors, as may easily be proved, by receiving

ceiving between the two polar wires of a pile, furnished with its electrometer, the current of vapour from an eolipile near the orifice, where it has its whole transparency, and is free from all mixture of vesicular vapour and precipitated water? When by a well-managed heat thoroughly dried soap is brought to a considerable degree of softness, this substance likewise undergoes a gradual change in its faculty of conducting the electricity of the pile; and the nearer it approaches a state of liquefaction, the more it loses the property of insulating the positive electricity in completing the circle between the two poles, so that ultimately we perceive evident traces of the decomposition of water in the interposed apparatus. Other substances lead to chemico-physical researches not less interesting. Sulphur is a non-conductor; so is its flame. Phosphorus and amber are both nonconductors; but their flames are conductors. Here is one anomaly. But how again are we to account for the difference in these two conducting flames? Why, in closing the circle between the two poles, does that of phosphorus insulate the negative effect, and that of amber the positive?

Soap when melted is a perfect conductor.

Sulphur and its flame conduct. Phosphorus and amber do not, yet their flames do, but opposite electricities.

It is very probable, that all these varieties of action are intimately connected with the chemical affinities of the two elements of the electric fluid; and we may flatter ourselves with the hope of some day obtaining results of importance, by sedulously varying and analyzing these facts. Lest however I should be accused of exaggerating the importance of these phenomena, in deferring their explanation, by way of concluding I will mention some hypotheses, which have formerly guided my researches, but which no longer appear plausible to me, since the facts that have presented themselves to me have become more numerous and diversified. I relate these only to show, that I have sincerely endeavoured to lay open the whole subject, so as to reduce it to a simple "is this all?"

Perhaps the phenomena may be traced to the chemical affinities of two electric fluids.

Do conducting flames, which in completing the circle insulate the negative effect, owe this property to a stratum of oil, which, formed of its elementary principles in the act of combustion, and deposited on the negative wire, renders it impermeable to the electric fluid? Carbon, hidrogen, and oxigen, exist in fact in most substances, which by their combustion

Hypothesis that oil is deposited on the negative wire.

combustion exhibit the phenomenon of negative insulation. It is natural too, that this oil-forming combination should be produced at the negative or hidrogenating pole, and not at the positive, where, on account of the oxidation that takes place, water and carbonic acid must rather be formed. Do not the fuliginous ramifications, that expand much more abundantly from the negative pole, owe their existence to this oleification, which detains them, renders them more compact, and feeds them by a continually renewed combustion; while on the positive side the more perfect oxidation causes them to disappear in gas and vapour, before they have been able to expand themselves?

Reasons why this cannot be the cause.

This specious hypothesis involves the following difficulties. The flame of the purest hidrogen gas insulates the negative effect. Now where shall we find in this the carbon necessary for the formation of oil? On inspecting a thousand times, even with a microscope, the negative and positive wires perfectly cleaned, and kept a long time in the flame of alcohol, I never could perceive the least difference between their extremities. Besides, on bringing together with the greatest possible dexterity the positive and negative wires in the flame itself, a spark is constantly perceptible. Farther, both the electrometers and apparatus for decomposing water show, that, the moment any filaments of the arborescent soot extend from one wire to the other, the galvanic circuit is completed; which would be impossible, if the negative wire were rendered impermeable to the electric fluid by any non conducting coating. Lastly, how can it be supposed, that such an insulating coat should be formed in a single instant over all the surface of a disk of several inches, held two feet above the flame? The cause of the phenomenon then, which all these facts have placed before us, is not so superficial, as the hypothesis supposes.

Hypothesis, that acid generated at the positive pole abstracts the alkali of the soap, and then leaves a coat of oil.

The following is an analogous hypothesis, which likewise I had formed respecting the mode of action of soap in insulating the positive effects. The positive wire of the pile is the seat of oxigenation, as the negative is of hidrogenation. If then the alkali of the soap be neutralized by the contact of the acidifying wire, the oil, or fat, will be set at liberty, and thus insulate the positive pole, the conductor of which

it

it surrounds. Indeed I have found, that, in an alcoholic solution of soap diluted with water, a manifest separation of the oleaginous base of the soap will be effected after some hours, and it will be deposited on the wire of the positive pole. In fact oil is set free.

This fact is certain: yet it is easy to show, that the hypothesis to which it serves as a base is not less manifestly in contradiction with several particulars of the phenomena, which it ought to explain. In reality, when several prisms of soap, connected together by intermediate arcs, are exposed to the action of the pile, there is no doubt a partial insulation with respect to each point of insertion that corresponds with the positive effect: but we cannot thence conclude, that this insulation is absolute, since the negative pole may be acted upon through all these prisms, and all the points of insertion of their conducting arcs, so as to take from it its charge. At the points of contact of the positive wires therefore there is no absolute obstacle to the passage of the electric fluid, and the hypothesis of an insulating coat of oil falls to the ground. Besides, in perfectly dry soap the positive insulating effect displays itself the instant it is applied, when no preceding chemical decomposition can have taken place. Yet this cannot be the cause of the phenomena.

On this hypothesis too how we shall explain the production of the same effect by the flame of phosphorus? Must we recur to a new hypothesis to account for this single fact, and say for instance, that here the oxiding action of the acid in the state of vapour, being produced with more energy at the positive wire, renders it impermeable to the electric fluid? But this hypothesis would be equally untenable, since platina wires exhibit the phenomena of positive insulation as well as any other metal; and this effect manifests itself the first moment of contact exactly in the same degree as after the long-continued action of phosphorus in ignition. Besides, on this supposition it would be difficult to explain, why sulphur does not produce the same effect. Farther difficulties in the way.

I am persuaded therefore, that these hypotheses are completely erroneous; that the cause of the phenomena we have discussed lies deeper, and is purely chemical; and that we The cause therefore remains to be shown.

we shall not be able to explain it, till these facts have been more thoroughly studied, than has yet been in my power.

Classification of substances with regard to electricity.

Meantime I would propose, for convenience, the following classification and nomenclature. All substances, applied to the poles of the pile are either, Class 1, *insulators*; or they are conductors. The latter are distinguishable into, Class 2, *perfect conductors*; and imperfect conductors. The imperfect are, Class 3, *bipolar imperfect conductors*: Class 4, *positive unipolar*: and Class 5, *negative unipolar*.

III.

Inquiries concerning the Oxidations of Iron; by Mr. Darso.

(Concluded from p. 280.)

Farther differences in the oxides of iron by sulphuretted hidrogen.

I DISSOLVED six grains of iron in muriatic acid without heat; and at the same time, in a separate vessel, six grains of red oxide, which I saturated with sulphuretted hidrogen. Four hours after I precipitated both these solutions by an alkali, and I found, that the precipitates of the green solution by sulphuretted hidrogen passed to red with the greatest rapidity. On pouring off the supernatant fluid, and letting water fall from some height on the oxide, it turned red immediately; but the precipitates of the other solution resist this trial. The green oxide by sulphuretted hidrogen, redissolved in muriatic acid, precipitates red; or at least it does so after two solutions. The common green oxides of iron, when recent, retain their colour even after being redissolved in acids five or six times. The reason of this no doubt is, that in the common green solutions of iron the hidrogen combines with the iron in the state of nascent gas, or very dense, and forms a more solid combination, than that into which the hidrogen furnished by sulphuretted hidrogen enters with the red oxide.

The green oxides hidrurets. This explains their alteration in the air.

If the green oxides of iron be hidrurets, as I suppose, it is easy to account for the alteration, that the green salts of iron undergo by exposure to the air. It is not to be wondered at, that hidrogen combined with oxide of iron should be volatilised spontaneously at a heat above 10° [54½° F.].

Almost

Almost all the combinations into which hidrogen enters are decomposed in the same manner, particularly when they are dissolved in water; as sulphuretted, phosphuretted, and carburetted hidrog n. All the vegetable acids likewise are decomposed spontaneously, when they are dissolved in water: and alcohol diluted in water is the same. The atmospheric air has no more influence in these phenomena, than it has in those of fermentation and putrefaction. All these operations require open vessels, because they evolve different gasses, which, if they were confined by any pressure whatever, would check the progress of the operation.

The experiments related in this paper I consider only as the outlines of a more extensive and deeper investigation; but as different circumstances have already obliged me to defer this research for one twelvemonth, and it is very doubtful how much longer it may be, before I shall be able to enter upon it, I was desirous of announcing these facts to the chemical world. These experiments lead to farther inquiry.

Corollaries deducible from the preceding Facts.

1. All the oxides of iron soluble in acids are red: and though their proportion of oxigen varies from 15 per cent to more than 50, they are not distinguishable from each other by any means hitherto employed in chemistry. Inferences from them.

2. The white oxide of iron is a salt with excess of oxide.

3. The green oxide is not a peculiar oxide, but a hidruret, or a combination of the red oxide with hidrogen.

4. The atmospheric air has no influence on solutions of iron, at least in the ordinary temperature of the atmosphere.

5. The saturation of iron with oxigen in its oxides does not destroy its magnetism, as hitherto has been asserted. Every oxide of iron is magnetic, or may become so without losing an atom of oxigen.

Note, referred to, p. 224. It has long been observed, that the magnetism of iron is weakened or disappears altogether in its oxides. At different periods this phenomenon has Magnetism of iron diminished or destroyed in its oxides.

Supposed causes of this.

has been differently explained, according to the manner in which metallic calces were considered. Previous to the

Loss of phlogiston.

pneumatic theory, the magnetism was ascribed to the presence of phlogiston. After the labours of Lavoisier had

Oxigenation.

shewn, that the formation of metallic calces was owing to the combination of oxigen with the metal, chemists naturally inferred, that the oxigen destroyed the magnetism: and as on the other hand facts seemed to prove, that magnetism was annihilated in oxides highly loaded with oxigen, it was established as a principle, that oxides of iron at a maximum, or red oxides, were not magnetic.

This inconsistent with some facts. Oxide at a maximum magnetic.

This principle, which does not agree with the fact I have just related, embarrassed several philosophers in explaining certain phenomena. The celebrated Baron von Humboldt, who discovered magnetic polarity in a serpentine, could not account for this property in a mineral, which appeared on analysis to contain only superoxigenated

This denied by Guyton.

oxide. On this occasion Guyton observed, that the term of superoxigenated, employed by the Baron, was inaccurate, for these two properties of being magnetic and superoxigenated were incompatible; and that the magnetism of the Saxon serpentine, and of other minerals which do not afford green oxide by analysis, should lead us to suppose

Hauy's mode of accounting for the restoration of magnetism to an oxide by heat.

intermediate oxidations of iron. Mr. Haüy, the learned natural philosopher, to whom magnetism is indebted for very perspicuous elucidations, has likewise suffered himself to be led away by the chemists; and, endeavouring to account for the magnetism, which some red oxides of iron acquire when strongly heated, says, that "this is owing to "the heat reducing some particles of the oxide, at the same "time that it assists the magnetic action of the globe, &c."

At first I subscribed to such respectable authorities, because, as Bacon observes, *oportet ediscentem credere;* and because, in the commencement of these researches, I tried several red oxides, obtained from different solutions of iron, as well as several aperitive saffrons of steel, which did not

Magnetic oxides with much oxigen.

give me the least sign of magnetism. But as I afterward perceived, that oxides greatly loaded with oxigen, or such as contained ·50 or ·56, retained their magnetism, while

others

others that had scarcely ·20 * were not attractable, I concluded, that there was some other cause acting at the same time with the oxigen, or perhaps exclusively, to destroy the magnetism. Reflecting on the circumstances, that preside over the formation of all these different oxides, I suspect, that in these phenomena, as in most of those to which the oxides of iron give birth, too much has been ascribed to the influence of oxigen, by referring to it effects in which it has no concern. If the loss of magnetism in some red oxides of iron be not exclusively owing to a state of extreme division, this at least has a more decided influence on it than the presence of oxigen.

Unmagnetic oxides with little.

The magnetism destroyed by division.

When the magnetic oxide, of which I have spoken, is precipitated by concentrated alkalis, and without the solution having been much diluted by water, the precipitate is a more or less blackish brown, it does not change by drying in the open air, and it is decidedly magnetic. If, on the contrary, the solution and the alkali be diluted with water, which has been boiled a long time to remove every suspicion of superoxidation, the precipitate is red, like all those called oxides at a maximum; and if it be dried in the open air, or by a gentle heat, like the preceding, it gives no signs of magnetism. Now we cannot ascribe this difference of colour and of magnetism to a different proportion of oxigen; for if we try the experiment with two equal parts of oxide, we shall find, that the weight of the red oxide is the same as that of the magnetic. The difference of magnetism therefore, like that of colour, depends on the difference of density, or the greater or less distance between the particles of the two precipitates.

Concentrated solutions throw down magnetic oxides:

dilute solutions do not.

In fact, when the solution is concentrated, the particles of the oxide touch each other, or at least are much nearer together, than when the solution is diluted with water: and this difference of approximation is in the ratio of the bulk of the two solutions, since the distribution of the oxide in both cases is uniform. Let us suppose, that the difference of approximation be in the ratio of one to ten; or,

This is owing to the state of approximation of the particles, which is in the inverse ratio of the quantity of the menstruum.

* Those obtained from the green solutions, of which I have spoken in the course of this Paper. See p. 273.

which amounts to the same thing, that the thickness of the columns of fluid separating the particles is $\frac{1}{10}$ of a line in the concentrated solution, and a line in that diluted with water; what will happen, if a drop of alkali fall on any point of the concentrated solution? The alkali will determine the precipitation of a certain number of particles of oxide, which will be at first $\frac{1}{10}$ of a line from each other, as when they were combined with the acid: but their specific gravity, assisted by the pressure of the atmosphere and of the solution, will be capable of overcoming the resistance opposed to their approximation by the little column of liquid that separates them. This is the reason why the precipitate is blackish, retains its magnetism, and at the expiration of a few minutes is insoluble in cold muriatic acid.

In the solution greatly diluted with water, though the alkali determines the precipitation of an equal number of particles, and though their specific gravity and pressure act in the same manner, as the resistance opposed to them by the columns of fluid is ten times as great, their approximation cannot be so complete. Hence the difference of colour, absence of magnetism, and facility of solution in acids.

The case is the same with oxides by calcination,

Besides, when extremely fine filings of iron are calcined, and divided by trituration in the course of the operation, till they have taken up 15 per cent of oxigen, we obtain a very fine red powder, much less magnetic than oxides with 30 or 40 per cent, obtained by the common process; that is to say, with common iron filings not triturated during the course of the process.

and red oxides when rendered magnetic by heat.

Finally, the red precipitates of solutions of iron, and most of the aperitive saffrons, after they have been well dried, exhibit no signs of magnetism. But if they be exposed to a strong fire for some time, their bulk diminishes, their colour is heightened, and they are decidedly magnetic. Now we cannot say here, that the magnetism is owing to a loss of oxigen, since the experiments of Proust, and more recently those of Berthollet, have proved that these oxides, exposed to the strongest heat of our furnaces, do not give out an atom of oxigen. To the same approximation is to

be

be ascribed the conversion of red crayons into magnets, related by Haüy in his elementary Treatise on Natural Philosophy; and the magnetic polarity, that displays itself in all the oxides of iron heated before the blowpipe, observed by Mr. Lelièvre.

That magnetism must be weakened by minute division of the substance deducted from the theory.

Besides the weakening of magnetism by division, and even the complete suspension of its effects, are consequences of our theory of magnetism. Though I am not acquainted with any accurate experiments, which prove, that magnetism acts in the direct ratio of masses, a number of facts attest, that it is subjected to this law*. Every one knows, that under similar circumstances a magnet eight inches long and an inch thick is more powerful than another of half these dimensions. The two hypotheses, that account for the magnetism of the earth, rest likewise on this law: for it is in consequence of the magnitude of their mass, that the action of the magnetic nucleus, or of mines of iron, extends to such prodigious distances. Without supposing this law, we cannot account for this phenomenon. Thus, all other circumstances being equal, a grain of iron will have a hundred times the magnetic power of $\frac{1}{100}$ of a grain, a thousand times that of $\frac{1}{1000}$ of a grain, and so on: and the imagination can easily conceive a subdivision, by which the magnetic power of a grain of iron would be so divided, and its sphere of action so shortened, that the magnetism of each particle should not only be unable to pervade the space that separates it from another, but even to exhibit any signs of magnetism, when brought into contact with a magnet. An example will elucidate this.

This illustrated.

Suppose I present the north pole of a needle to a particle of iron filings. The austral fluid of this particle will place itself at the extremity nearest the needle, while its boreal fluid will be expelled to the opposite extremity. But as there is a sufficiently appreciable difference between the distance at which the north pole of the needle acts on the two fluids of the particle of iron, the south energy of this will overcome the north, and through this preponderance it will approach the needle.

* And even though this law should be slightly modified in some way, the effects of this modification would be of little account in the present discussion.

Now let us continue to subdivide this particle, till the distance between the two poles of its molecules, brought into contact with the needle, shall be so small, that the seats of the two poles shall be as we may say compounded together: the difference between the attraction and repulsion will then become inappreciable, and the molecule will give no signs of magnetism.

Restoration of magnetism to an oxide not the effect of caloric,

It may be said, that this development of magnetism in oxides strongly heated is rather owing to the action of the heat weakening the coercive power that opposes the magnetism. But beside that this coercive power is not a fact so certain as the approximation, that these oxides undergo whenever they become magnetic, it can account only for part of the phenomena, since in oxides by precipitation, which can be obtained magnetic at pleasure, heat has no concern.

for it takes place without heat.

Whether oxigen may weaken magnetism is not determined.

For the rest, whatever be the cause of this phenomenon, it is proved, that oxides saturated with oxigen are magnetic, or at least may become so without losing an atom of oxigen. I do not however mean to assert, that a given quantity of iron saturated with oxigen retains the same magnetic power as it possessed before it was oxigenized: for on this subject I have made no experiments.

IV.

Description of a Curvilinear Saw, invented by JOHN TROTTER, *Esq., of Soho Square, from whom the following Communication was received*.*

GENTLEMEN,

A curvilinear saw very desirable

WITH the view of obviating many difficulties and expenses, which have long attended the operations of those requiring curvilinear sawing in their trade, and of public bodies connected with those trades, through the licentious and refractory conduct of sawyers, it has been represented to me as a measure extremely desirable, to adopt more ge-

* From the Transactions of the Society of Arts, &c. for 1806, who voted their gold medal to Mr. Trotter for this invention.

nerally

nerally mechanical powers, could such be discovered as would preclude much mystery and manual labour.

Considering the subject in a national point of view, as connected with our naval yards in the formation of timber; with our military departments, in respect to wheels of every description; with our whale and herring fisheries; our public and private breweries and distilleries; our East and West India Companies, and other bodies depending on cooperages, as well as other minor trades peculiarly liable to the evils complained of; I invented a curvilinear saw, which, with little aid of the most ignorant labourer, answers every purpose. on several national accounts.

Having effected these ends, suffer me to solicit the honour of your acceptance of a model, together with a drawing of my saw, sufficiently accurate for the use of those in remote situations to work by, who may wish to use or make them.

I have the honour to be,
Gentlemen,
Your most obedient and most humble Servant,
JOHN TROTTER.

Soho Square, Sept. 12, 1805.

To the Society for the Encouragement
of Arts, &c.

Reference to the Engraving of Mr. Trotter's Curvilinear Saw. Plate X.

Fig. 1. Represents a bird's eye view of the saw and machinery. The dotted lines show the spindle *a*, moving on two centres *b*, *b*, having at one end a pulley *c*, and at the other a concave saw *d* (with a corresponding convexity to the curve required to be sawed,) secured on the convex side by a collar, and on the concave side by a loose collar, and screw nut. Description of one.

e, *e*, Two grooved plates, admitting through the top of the bench and fence *f*, screw bolts fastened by thumb nuts, by means of which, and a parallel motion *g*, the fence *f* is regulated, and consequently the conductor *h* of the wood *i* admits it to be sawed through, as represented in the dotted line at any part required.

The fence, conductor, and saw, must all be curved alike; but to saw in smaller circles, with the same saw and at the same time square at the face of the bench, a steel slider *k*, regulated by two screws, is made to press, as occasion may require, on the convex side of the saw, and raise the vertical line of it to a right angle with the bench; otherwise the top of the bench itself must receive the same inclination to the vertical line of the fixed saw.

Fig. 2. Is a front view of the saw and bench, in which the teeth of the saw are more clearly shown.

Fig. 3. An end view of the same machinery.

Fig. 4. Shows the saw, axle, and pulley, all made of iron or steel, and separated from the frame.

V.

Account of a Bookbinder's Cutting Press, for which Fifteen Guineas were voted to Mr. JAMES HARDIE, *of Glasgow, by the Society of Arts*.*

SIR,

The press more powerful than the common, and saves time.

I HAVE herewith sent a model of an improved press for bookbinders, the invention of Mr. James Hardie, bookbinder, Glasgow. The inventor claims no other merit than that of having simplified the common press, rendered it more powerful, and adapted it to work more economically; or, in other words, to save time to the workman. It has been found so superior to the press in common use, that all the bookbinders in Glasgow and Edinburgh are adopting it. This is perhaps the best proof that can be given of its utility. The inventor has received certificates from the bookbinders alluded to, which will be sent to the Society, if they think the press worthy of their notice. Mr. Hardie, in desiring me to submit the model to the inspection of the Society, has in view chiefly to benefit the bookbinders in places remote from his residence, an object which he thinks cannot be so well attained in any other way, as by the publicity which the Society is able to give to improvements deserving of its notice.

Generally used at Edinburgh and Glasgow.

* From their Transactions for 1806.

M. I. Rollers Curvilinear Saw.

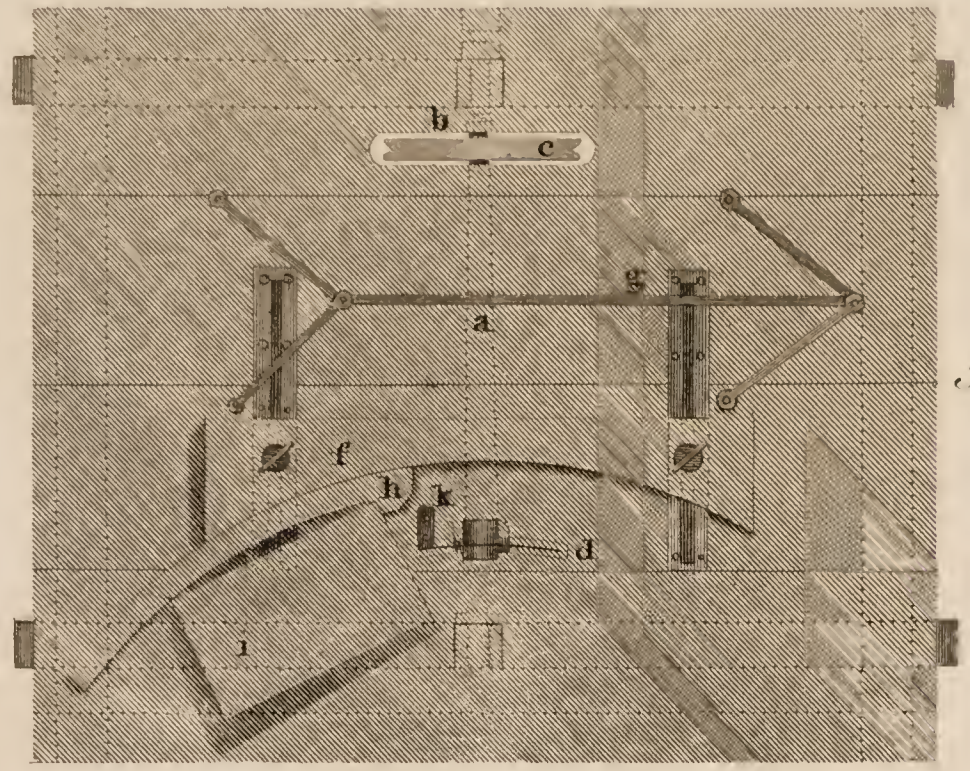
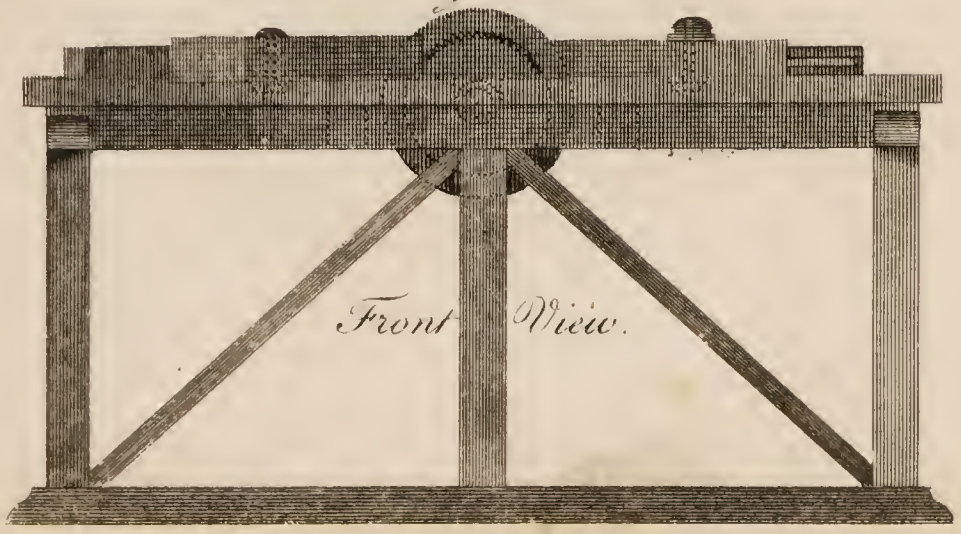


Fig. 1.

Fig. 2.



M. I. Hardies Book Binder's Cutting Press.

Fig. 5.

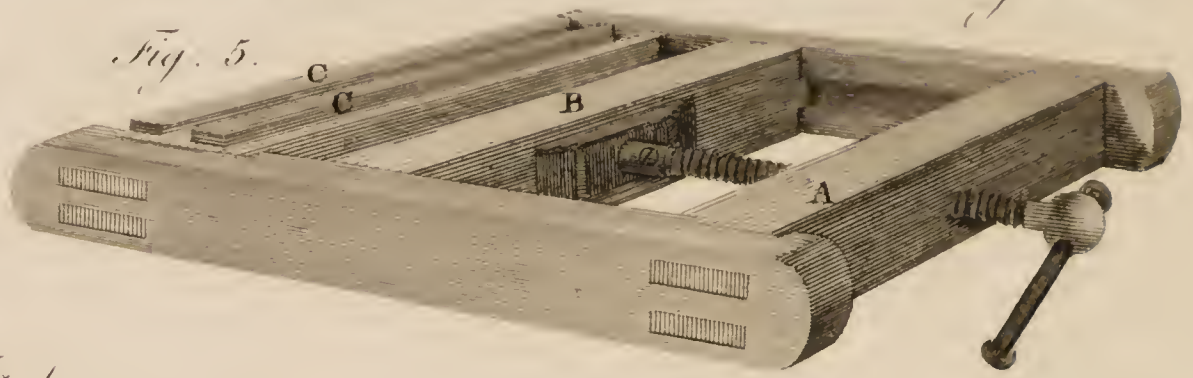
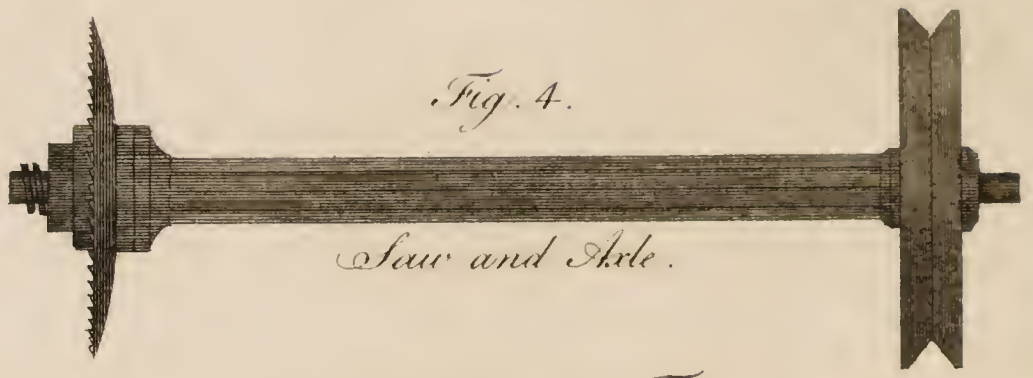
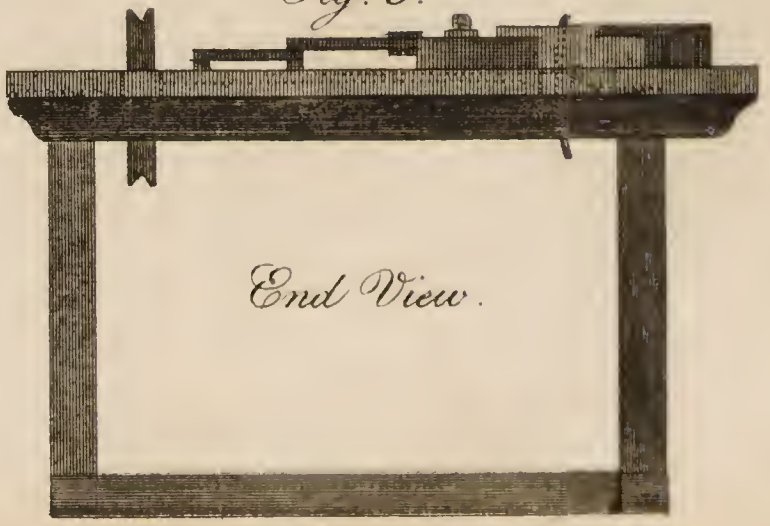


Fig. 4.



Saw and Axle.

Fig. 3.



The improvement of this simple instrument has cost Mr. Hardie much time, and even expense; and he will be glad to receive any remuneration from the Society which they may think his invention deserves.

I am, Sir,

Your most humble Servant,

A. TILLOCH.

To C. Taylor, M. D.

Twenty-three persons testified by their signatures and subscriptions to Mr. Hardie their approbation of his Cutting Press.

Reference to the Engravings of Mr. J. Hardie's Bookbinder's Cutting Press. Plate X. Fig. 5.

Description of the press.

The principal difference between this and the press which has been from time immemorial employed by the bookbinders consists in effecting the business by one iron screw, instead of two wooden ones formerly used. This screw works in a nut let into and screwed to the top piece A, its lower end working in a collar, screwed to the moving piece B, sliding in grooves within the two sides of the frame. C C are the guides for the plough, as in the common press.

VI.

On Blende, and some other Articles; by Professor Proust*.

The zinc in blende not an oxide:

THAT zinc is incapable of disputing oxigen with charcoal, is a known fact: and the same may be said of sulphur. Consequently, if blende contain oxigen, it must yield it to the action of charcoal.

I kept a mixture of transparent yellow blende and fir charcoal at a red heat for an hour, but I did not find the slightest indication of sulphurous acid. The mixture being washed, to separate the charcoal, left the blende behind, which had undergone no change. Where then is the oxigen of blendes?

* Journal de Physique, Vol. LXIV. p. 150, Feb. 1807.

I have

and the metal in it saturated with sulphur.

I have heated redhot a hundred parts of the same blende with as much sulphur; and, when the operation was ended, it had not increased a single grain, or even changed colour. Hence we may conclude, first, that the metal in blende is saturated with sulphur; and secondly, that it is free from oxigen, otherwise the sulphur is a combustible, which would have taken its oxigen from it. The following experiment does not allow me to doubt this.

Artificial sulphuret of zinc,

I heated together a mixture of sulphur and pure oxide of zinc, a hundred and twenty-five grains of each: the produce was a hundred and thirty-six grains. Apprehensive however, that it might not be saturated, I heated it with fresh sulphur, by which it was increased to a hundred and thirty-eight grains. On heating it with sulphur a third time, it did not go beyond a hundred and thirty-eight. I repeated the experiment twice more, and the product stopped at a hundred and thirty-eight grains. Hence we may infer, if there were no mistake, that 38 parts of sulphur took the place of 25 of oxigen, that were condensed in the oxide. It would be superfluous to say, that this process evolved torrents of sulphurous gas.

38 parts sulphur to 100 of zinc.

Morveau is I believe the first, who reproduced sulphuret of zinc by heating its oxide with sulphur. The artificial blende remains pulverulent: but it appears to me by Morveau's account, that it is capable of being melted by a strong heat. Blende is transparent; hence, they say, its metal must be oxided. But the sulphurets of mercury and of arsenic are transparent likewise; yet they are free from oxigen. The sulphuret of arsenic, I know not whether I have mentioned the fact, supports any temperature to which you choose to expose it, without affording any indication of sulphurous gas, or losing its transparency. Arsenic acid or oxide gives out sulphurous gas in abundance, when heated with sulphur, and affords a transparent sulphuret, similar to that produced by the metal itself. These compounds therefore contain no oxigen; and consequently transparency is no argument for the oxidation of a sulphuret. But why should zinc refuse to unite directly with sulphur? I confess I see no reason for it. I had intended

Transparency of a sulphuret no proof that the metal is oxided. Sulphuret of arsenic.

to

to treat zinc with cinnabar, and other sulphurets; but different objects have prevented me.

The sulphuret of zinc is frequently concealed by foreign oxides and sulphurets: hence red, black, ash-coloured, green, and other blendes, of which so many species have been made. Now this is precisely the same, as if, in the natural history of wool, different species were to be made of those that are dyed red, black, gray, or green. Different colours do not constitute species in blendes.

There are blendes coloured by red oxide of iron, which appear black; but their powder is red. These may be analysed by muriatic acid, which will cause the iron to descend to its minimum of oxidation, on account of the sulphuretted hydrogen formed during their solution. We should be aware of this, that we may not suppose the oxide to be at a minimum, where nature has placed only oxide at a maximum. Black blendes coloured by red oxide of iron.

There are some that contain lead in the state of oxide, or of sulphuret. If these be exposed to the action of muriatic acid, the whole of the lead is found in the solution: but if oxigenized muriatic acid be used, we must look for the lead in the residuum only. The reason of this is obvious; the sulphur of the blende, being acidified, precipitates the lead in the state of sulphate. Blendes with lead.

Hidrosulphuret of Zinc.

Sulphuretted hidrogen precipitates zinc from its solutions in a yellowish white powder, which is a hidrosulphuret. This precipitation however is limited. When the acid is freed from a considerable portion of the oxide, so as to be in excess, it disputes the remainder with the hidrogen, and the precipitation stops. It is necessary therefore, to add a little potash, to neutralize this excess. The alkaline hidrosulphurets produce the same precipitate. The nitric acid acts with vehemence on this hidrosulphuret, burning its hidrogen, and part of its sulphur. Muriatic acid applied cold expels the sulphuretted hidrogen in abundance. This hidrosulphuret at a red heat gives out water and sulphurous acid, and is converted into a simple sulphuret, or blende. The sulphuret of zinc, whether native or artificial, yields sulphuretted hidrogen; which however is not an educt, but a product. Hidrosulphuret of zinc.

Ambergris.

Ambergris.

Yellow ambergris from Brasil.

This piece was found on the coast of Brasil. It is of the colour of honey; very homogeneous in its texture; and free from those fragments or beaks of the cuttlefish, that are interspersed in the ash-coloured amber of the shops. Alcohol dissolves it entirely, except a few slight pellicles. This solution is curdled by water. Evaporated it leaves a yellow substance, which softens and burns like a resin; swims on a solution of potash, which dissolves but a few atoms of it; and gives out no smell of ammonia. The fragrance of this purified resin is still that of the ambergrise itself. Exposed to distillation it melts quietly, without swelling up, and yields a thick, yellow oil, which swims on water. It is accompanied with some indications of an acid: but what is astonishing is the ambergris scent of this oil.

Cochineal.

Cochineal acid. Its colouring matter precipitated with lime,

but obtainable more pure with oxide of tin or lead.

The powder of this insect has always seemed to me to have an acid taste. I know not whether it be the effect of the action of air on any of its principles. Lime water precipitates its colouring matter completely, and the result is a lake, on which alcohol has no action. To obtain the pure colouring principle, we should decompose this lake: but as the white oxides of tin and of lead likewise saturate themselves with it readily, we may obtain it still more pure from these by means of sulphuretted hidrogen, than by employing acids. I believe the colouring principle of kermes likewise precipitates with lime.

Ox gall.

Resin of gall.

Acids precipitate from this a resin, which, after it has been well washed in boiling water, may be drawn into threads like boiled turpentine. When dry, it is semitransparent, greenish, melts with the gentlest heat, and on burning coals exhales a smoke that has somewhat of a fragrant smell. Alcohol dissolves it, without leaving any residuum; and water precipitates this solution. Oxigenized muriatic acid whitens it with the assistance of a gentle heat. It remains in complete fusion, after having given out some moisture:

moisture: and a strong heat causes it to rise in the form of a thick oil, the smell of which is that of an animal substance, and unpleasant. A little carbonate of ammonia accompanies it. It leaves very little coal. But what distinguishes it from the aromatic vegetable resins is its great solubility in the weakest alkalis. Acids separate it from these without alteration, and it may be drawn out into threads as before; so that we cannot deny it the principal characters of resins, but it is not always the same. I have obtained some from gall, which was soft, semifluid, or incapable of assuming the consistency of the preceding.

Soluble in alkalis.

Not always the same.

To obtain this resin pure, we must begin with passing dried gall through alcohol, to separate its albuminous portions. These occasion the putrefaction of gall when kept: but the extract, passed through alcohol, and evaporated to the consistence of a sirup, is no longer susceptible of alteration; and it is in this state I keep it for my lectures*.

Mode of obtaining it pure, and preserving gall from putrefaction.

VII.

Remarks on the Structure of Mount Jura, from a considerable Number of Heights taken by the Barometer, and extended through France to the Sea; by Mr. And. de Gy, *Member of the Academy of Cassel, &c.* †

1. THE four loftiest summits of the first chain of the Jura are nearly on a level, as is their base, the lake of Geneva, on a length of twelve leagues.

First chain of mount Jura.

The summits of the Suchet and Sucheron, which rise from the western extremity of the lake of Neufchâtel, six leagues from the preceding, are 30 toises lower. The other sum-

* An ingenious artist, Mr. J. Clark, in his Instructions for Drawing and Painting in Water Colours, observes that gall, which it is frequently necessary to add to a tint, when it will not adhere uniformly to the paper, from any slight greasiness of its surface, will keep much better, if it be boiled a little. This no doubt is owing to the coagulation and separation of the albuminous matter by heat, conformably to the remark of Professor Proust. W. N.

† Journal des Mines Vol. XVIII. p. 430.

mits

mits continue diminishing to the Rhine; but to the Lebreberg, five leagues below Soleure, or through a space of 25 or 30 leagues, they decrease almost imperceptibly, as does their base. In this interval indeed the summit of the Chasserale is not above 40 toises lower than the four highest; but from the Lebreberg to the Rhine the declivity is more rapid.

The plains or table-lands, that form the western foot of this first chain of the Jura, likewise diminish in height proportionally to the mountains, and in the same direction.

Second chain. 2. The highest summits of the second chain of the Jura continue nearly on a level with each other as far as the Stierberg, opposite the Lebreberg, a distance of 20 or 25 leagues: but thence to the Rhine they also lower more rapidly, like those of the first chain.

Third chain. 3. The highest summits of the third chain are nearly on a level with each other throughout its whole length, which is 30 or 35 leagues.

Fourth chain. 4. The highest summits of the fourth chain, which are nearly in a right line from Estival, four leagues north of Moirans, to St. Hippolytus, a distance of 20 or 25 leagues, are likewise nearly on a level; but those to the south of Estival, and to the west of the above line, follow the inclination of the rivers.

There are several large plains that intersect this chain, particularly above Ornans and Villafans. These plains are nearly level throughout.

Fifth. 5. The fifth chain is very similar to the fourth.

Sixth, or lowest chain. 6. The lowest chain of the Jura is about 60 leagues in length. Its loftiest summit is that called the *haut des Tronchats*, three leagues east of Porentrui. From this point the other summits diminish on both sides, according to the course of the rivers. There is no great difference from the mountain of St. Ursane to the Mont de Triève, two leagues north-east of Beaume: they afterward diminish more, and remain on a level with each other to the mountain of Pouillat, three leagues north-east of Bourg-en-Bresse, that is for 35 or 40 leagues.

The plains at the eastern foot of the last summits are also nearly on a level with each other, particularly from Orgelet to Cernans, above Salins.

From

From this arrangement it follows, that the whole of the Jura forms an amphitheatre of 60 leagues long, by 12 or 14 broad, the steps of which, or chains of mountains, that rise above one another in a line from north-east to south-west, are level, each almost throughout its whole length. The whole forms an amphitheatre.

7. In the chain that separates the waters that fall into the two seas, the loftiest summits, except the Haute-Joux l'Aiguillette, are in the neighbourhood of Dijon. From this point, following the chain to the south-west, the highest summits are nearly on a level; but the other summits of this chain decline to the north-east. Chain that divides the rivers of France.

Dijon and Besançon are level with each other, and the highest summits in the neighbourhood of these two cities, separated by a plain of 14 or 15 leagues and a few hills, are likewise on a level. The summit called Tasselot, the highest near Dijon, is 306 toises above the sea: the rocks of Montfaucon, the highest near Besançon, are 303 toises above the sea. The other summits to the south-west of Dijon and of Besançon, in the two chains in which these cities are situate, are likewise on a level in their other corresponding points on each side of the Saone, at 12 or 13 leagues distance, and in a length of 20 leagues.

8. If all the valleys were filled up to the height of the summits of the mountains that border upon them, from the sea to the tops of the Alps, a very gently inclining plane would be formed. For instance, if a right line were drawn from Havre to the loftiest summits of the mountains, that form the groupe of St. Gotthard, it would cut the intermediate chains of mountains nearly at right angles, passing two leagues south of Paris, between Langres and Dijon, five leagues north-east of Besançon, two leagues north of Neufchatel, and a little to the west of Berne. General level from Havre to the summit of the Alps.

This line would be 160 of the old common French leagues in length [445 miles], and the whole ascent would be only 1750 toises [3733 yards], the height above the sea of the Glaetscherberg*, the loftiest summit of these mountains; so that if the declivity were uniform, it would be a little

* The height here given is not from actual measurement, but estimated from a comparison with them that were measured.

less

less than 11 toises per league. But, as the whole slope is not uniform, to give a more accurate idea of it, we may divide the 160 leagues into five unequal portions, each of which would have a nearly uniform slope.

Level from Havre to Dijon.

1st. From Havre to the chain that separates the waters falling into the two seas, taken near Dijon, a length of 95 leagues, with 300 toises elevation above the sea, which would give an ascent of three toises, one foot, four inches, in every league.

From Dijon to Besançon.

2d. From these summits to those of the lowest chain of the Jura, beyond Besançon, at both which places the height above the sea is the same, making a horizontal line of 23 leagues.

From Besançon to the top of the Jura.

3d. From the highest summits near Besançon to those of the highest chain of the Jura, at Casserale, two leagues north of Neufchâtel, a distance of 12 leagues, with 550 toises difference of elevation, giving $45\frac{10}{12}$ toises rise in a league.

From the Jura to the first chain of the Alps.

4th. From the loftiest summits of the highest chain of Jura to those of the lowest chain of the Alps, south-east of Berne, a distance of 15 leagues, with 200 toises difference of elevation, giving 13 toises two feet rise in a league.

From the lowest Alps to St. Gotthard.

5th. From the summits of the lowest chain of the Alps to the highest of those of St. Gothard, 15 leagues distance, and 700 toises of elevation, being a rise of 46 toises four feet in a league.

VIII.

Some Mineralogical and Geological Observations, made in the Isle of Arran; by the Rev. James Headrick*.

ADVANCING along the north shore of the island from Loch Ransa, the first remarkable thing that occurs is at Craig-na-Srone *(Nose Rock,)* where the secondary strata are seen resting upon the micaceous schistus. The first

* From that Gentleman's View of the Mineralogy, Agriculture, Manufactures, and Fisheries, of the Island of Arran.

stratum

stratum of this kind is a sort of chalky limestone, which contains rounded pieces of quartz, fragments of schistus, &c., so as to constitute a species of pudding stone. Many parts of the stratum contain few of these extraneous bodies, and would make excellent lime. Further on, a sort of pudding stone, red sandstone, and shiver, occupy the coast. Calcareous breccia.

Where the limestone rests upon the schistus, three whinstone veins intersect the schistus, and the strata that rest upon it. Where these veins run in the schistus, the cheeks on each side are penetrated by the whinstone; and fragments of the schistus are found immersed in the whinstone veins. One vein divides into two, like the letter Y; and pieces of slate are found immersed in the whinstone, at the angle of separation. Schistus intersected with whinstone.

The Scriden rocks are strata of breccia, or puddingstone, of most enormous thickness, and leaning upon the side of the mountain at an angle of about 45°. About a hundred years ago, immense masses fell from these rocks, and now encumber the beach, rendering it difficult and dangerous to pass along shore. The concussion shook the earth, and the sound was heard in Bute and Argyleshire. On climbing towards the summit, found the whole of this enormous mass of strata, which reaches almost to the top of the mountain, had shifted from its original position; and that its transverse sections were separated from each other, so as frequently to leave spacious gaps between. In many cases too, the upper masses ride upon the ends of those below them; which, having no visible support, excite the most lively apprehension that the whole is about to fall, and crush the beholder to atoms. The Scriden rocks are reckoned the extreme point of Arran, towards north-east. Rocks fallen from the mountain.

Red sandstone, with sometimes pudding stone, continue along the coast, and ascend to the top of the mountain where they meet the schistus. Sandstone and puddingstone.

Near the Cock, these strata are intersected by a great vein of porphyry, the south-east side of which consists of rhomboidal, sharp-angled crystals of feldtspar, inserted in a red ground. On the opposite side, the ground is mostly intersected by porphyry.

blue basalt, with some streaks and spots of red intervening. This vein seems to rise to the top of the mountain; and it exhibits a curious specimen of different species of porphyry combined in the same mass. I do not see how the igneous theory can account for it; because, had it been raised from the bowels of the earth in fusion, it ought to have been homogeneous in its structure. The variety and separation of materials show they had been conveyed into a chasm in the strata, at different times, and from different sources; where they consolidated, by the attraction of cohesion, which operates upon bodies whose particles are brought into close union, by extreme pulverization. The earths, too, when minutely pulverized, combine with water, and pass from it, either in the form of cement to unite the grosser particles, or in the form of crystals. One point is clear, that the feldtspars in this porphyry had not, like those we have so often described, been conveyed to their present situation in a solid state, because their angles are not blunted, or worn. They must therefore have consolidated where they are now found.

Different species of porphyry in one mass.

The Cock is not a solid rock, as I expected, but a huge mass of sandstone that has fallen from the rocks, and stands on a narrow base upon the beach. It has acquired this name from mariners, to whom it serves as a land-mark, and to whom it presents a very lively representation of a cock crowing, and clapping his wings.

Cock, a huge mass of sandstone.

Contiguous to the Cock are irregular veins of basalt intersecting red sandstone, of a dark blue colour internally; but the sides next the sandstone are of a deep red colour, smooth and glossy like Roman bricks. The breadth of these veins varies from about one to three or four inches. Further on, the same sort of veins occur in red indurated clay shiver.

Red sandstone intersected by basaltes.

Several massy strata of indurated clay occur, of a florid red colour, to which the sea has conveyed a fine polish. From these, I am confident, vases, urns, jars, and all the more durable species of earthen ware, might be fabricated, susceptible of a jaspidean polish; and, being variegated with spots of different colours, might emulate the most elegant porphyry.

Red indurated clay, capable of a fine polish, and being wrought into ornamental vessels.

Within

Within tide are several strata that look like a Mosaic pavement of finely polished brick, of a florid red colour. They consist of equilateral pieces, or rhombi, of indurated clay, divided by septaria which run in straight lines parallel to each other, and are crossed by other straight and parallel lines with the greatest regularity, so that each rhombus is enclosed within septaria, which separate its sides from those of other rhombi, which are parallel to them. The breadth of the rhombi may be about fourteen inches; that of the septaria about half an inch. The colour of the septaria is a pale red, inclining to whiteness; and it looks very like puzzolana cement, in the interstices between regularly formed stones. In many cases the pavement is worn down; but the cement, which is harder, preserves a regular equality of height. Natural Mosaic pavement.

I doubt much if the most skilful mason, or even a mathematician, could produce any thing more regular, or more beautiful.

Here are numerous strata of red clay shiver, and of red slaty schistus, which include various stratulæ of hæmatites, and of kidney ironstone. The hæmatites is generally arranged in stratulæ, but often interspersed. When stratified, it generally comes off in rounded pieces, whose sides are perpendicular, and are thicker towards the centre, than towards the circumference. When interspersed, the pieces are generally broader, are of a round, or oval form, and flattened towards the circumference. Iron ores.

The pieces that have been long exposed to the air are of a blood-red colour, though darker towards the centre. But by a little digging, pieces are found, with a red tegmen, but internally of a fibrous texture, the fibres at right angles to the breadth of the stone, and the colour that of steel. The interspersed pieces often exhibit an indented fracture, where the surface of indentations has the lustre of steel.

The kidney-form pieces often exhibit a dull red earthy appearance to their centre. But, where they have not been too much exposed to the atmosphere, they discover a radiated texture, with the lustre of steel.

These two species feel very heavy; but there are also

other stratulæ, which, though good iron stone, seem to contain a proportion of clay, and do not feel so heavy.

Used for marking sheep.

The people here call this ironstone *keel*, and use it for marking their sheep, and various other purposes. It has a greasy feel, and gives a stain to the hands, which it is difficult to wash off.

The farm of Cock is on a steep bank projected from the side of the mountain. The soil is mostly of a blood-red colour, and is composed of the debris of the schistus, and of the ironstone which it includes. It is wholly encumbered with this ironstone. Where burns had made excavations, I traced these ironstone strata to the summit of the mountain.

Imbedded in clay, under peat, on the summit of the mountain.

What is most extraordinary, great quantities of this ironstone are found, imbedded in clay, below peat bogs, on the flat summit of the mountain. This clearly shows that the summit was formerly covered by strata of red schistus, including ironstone, which have mouldered down. Much of the clay may have been washed away, leaving the ironstone, which water could not easily carry from a flat surface.

Blood-red limestone.

Connected with the latter, are several strata of limestone, of a florid brick, or blood-red colour. Such limestone always occurs, where hæmatitical strata present themselves along the coast. We shall therefore take no further notice of this limestone, until we reach Currie, beyond which it was not observed.

Jaspidean marble.

A stratum about three feet in thickness, and formed into blocks of from three to four feet in length, attracts attention. This stone contains innumerable shells, chiefly madrepores, some of which exhibit a bright crimson colour. The ground of the stone is somewhat calcareous, and in its fracture every way resembles jasper, of a chocolate colour. It may, not improperly, be called jaspidean marble. Were it polished, it would exhibit a striking appearance.

Change to sandstone.

Advancing onward, the strata upon the beach are suddenly changed. White and grey sandstone, with sometimes a mixture of yellow, occur, and are continued a great way.

In these the Cock coal is included. It is confined within an angular space, formed by two ledges, or edge-seams of limestone, one from north-east, the other from south-east, which meet at right-angles. To the north and south of these ledges of limestone, though the white sandstone extends a long way, and includes numerous beds of black bituminated shiver and blaes, no stratum of coal has been found. Coal.

There are three or four seams of coal running parallel from north to south; the principal, or main seam, being about fourteen feet in thickness. They dip nearly towards north-east, at an angle of 45°. Pits were sunk, and efforts used to work this coal, about fifty years ago; but as there is no harbour to export by sea, and a road must be cut several miles through rocks, a salt-pan was built, to consume the coal in the manufacture of salt. The undertaking seems not to have succeeded, and was soon abandoned. Attempts to work it fifty years ago.

The coal is of the same species with that at Kilkenny in Ireland; and there are similar strata in Ayrshire, in Fifeshire, and various parts of Scotland. It is blind coal, of uncommon excellence. It is not so apt to fall into powder as most other species, and, when fresh dug, it exhibits a metallic lustre. It is hence called *glance* coal by some; but this word does not distinguish it from some of its own varieties, which have no lustre; nor from some species of bituminated coal, which have a shining appearance. The word *blind* coal is more expressive of its peculiar property, in emitting neither flame nor smoke; as it consists of carbon, without any impregnation of bitumen. A blind coal like that of Kilkenny, of uncommon excellence.

I could not learn that the working of this coal was abandoned, because it 'soon disappeared,' as stated by Mr. Jamieson, p. 101; but that, from its inaccessible situation, they could not work it with profit. Boiling salt could hardly absorb their refuse, far less such a quantity as would keep the pit constantly going. It is well known, that if there be not a demand sufficient to absorb all that is turned out, no coal can be worked with profit; and that the working cannot be abandoned and resumed, according to the fluctuations of the demand; because, in that Why discontinued.

that case, men's wages would be running on, the machinery would be rotting, while the pit would be drowned with water.

Mr. Jamieson also states 'the great frequency of basaltic veins as another cause, which must render the coal, if it should ever be detected, of an indifferent quality, and difficult to work.'

No basaltes in the coal field.

With regard to the basaltic veins, I remarked it as an uncommon circumstance in Arran, that I could not find a single basaltic vein in the coal-field, or as far as the white sandstone extended on each side of it. There did not therefore appear to be the smallest ground for believing that the coal was cut off, or its quality injured, by basaltic veins.

Mistake of the miners.

When they wrought the main seam, by digging along its outcrop a large open trench, they came to the strata which form the steep side of the mountain, and which here rise at an angle of nearly 70°. They thought the coal extended through the base of the mountain, and cut a mine to follow it out. Had they bestowed the slightest attention, they might have seen, that the strata on the side of the mountain are very different from those which include the coal; and that, when the latter touch the former, they suddenly terminate. The strata on the side of the mountain appear to have been of much earlier formation, and they rise at a much higher angle, than those which include the coal. Nor does it appear that the quality of the coal was in the least affected by its approach to the mountain; for it was equally good until it was cut off.

Siliceous sandstone bent into Gothic arches.

On examining the strata perforated by their mine, I found them to be siliceous sandstone flags, of great hardness, and of a brownish white colour, their surfaces exhibiting micaceous scales. They are from one to two inches in thickness, and are curiously bent upwards, into the form of a Gothic arch, or rather of a great many arches overlapping each other, which form the roof of their mine. It was useless to follow the coal from the strata where it was found, into strata so very different, both in quality and position. As far as I could learn, the coal only failed them here, where they had no right to expect it.

Course of the coal, and man-

From the position of this coal, there can be no doubt but

but it expands to a great extent below the sea. If ever it be convenient to work it, I conceive it should not be by sinking pits, but by sinking in the stratum itself, and drawing it up the inclined plane of sandstone, on which it rests, by carriages running upon rollers. ner in which it should be wrought.

This coal, being esteemed pure carbon, and capable of producing a most intense heat, I am surprised it never has been applied to the smelting of iron, and other fornacic uses. The ironstone here seems inexhaustible, and of excellent quality. A harbour might be scooped out of one of the ledges of limestone, which enclose the coal-field; which would cost nothing, as the limestone would repay the expense with profit. From this, the coal and ironstone might be exported to a situation favourable for an ironwork. With the ironstone might be made profitable.

Some strata of bituminated shiver, or shale, of a black colour, are visible, not only in the coalfield; but many are visible, for a great extent, on each side of it. These strata sometimes throw out an efflorescence of sulphate of magnesia. Bituminous shale.

But in one of the seams of coal which Mr. Cowie had wrought in the way called *open stretch*, for the purpose of burning limestone, I observed a highly bituminated species of till, of a black colour. This appeared so extraordinary, that I requested Mr. Cowie to write down a description of it, which follows. A stratum highly impregnated with bitumen between two seams of blind coal.

'The seam of coal, at the Cock of Arran, wrought by James Cowie, is first about ten inches, then eight or ten inches of a dauchy till, then twenty inches of coal. The dauch which separates the two seams of coal, is arranged in this manner like the back-bone of a fish, and rises in large pieces, but parts in the middle. The till between the two seams of the coal serves to burn lime.'

Mr. Cowie added, that the dauch was always mixed with the blind coal in burning lime. That it seemed to kindle more readily than the coal; and the only difference was, that the dauch always left a large *guest* (cinder); whereas the coal burnt into a fine white ash, of very small quantity. Mixed for burning lime.

Here, then, are highly bituminated strata of clay, not only connected with blind coal, but one interposed between two seams of that fossil.

Remarks on the vulcanean theory.

Our men of fire make their favourite element operate as many contradictions, as the hocus-pocus tricks imputed to phlogiston by the older chemists. At one time, phlogiston could not penetrate the most porous bodies; at another, the most dense were insufficient to confine it. At one time, it was the cause of gravity and attraction; at another, of levity and repulsion.

These gentlemen assert, that blind coal has had its bitumen evaporated, by the great heat which elevated the strata, from want of sufficient pressure to confine it; and that bituminated coal retained its bitumen, while subjected to this heat, in consequence of the enormous pressure which prevented its escape.

How came the bitumen here on this hypothesis?

But I would ask these gentlemen—How came the clay strata, in the same alternation with the coal, to retain their bitumen, while the coal was deprived of it? But especially, how came a stratum of clay, included between two stratum of coal, to retain its bitumen, while both the stratum of coal lost theirs? I do not see how these gentlemen can answer these questions, in a way consistent with their theory.

None in the sandstone over the blind coal.

They refer us to the sandstone which covers the blind coal, and allege we shall find some traces of the bitumen there. But though I examined the sandstone strata which formed the immediate roofs of the strata of coal, and many others, with the utmost care, I could not find the smallest visible trace of bitumen in them: nor could I trace the slightest mark of vegetable impression, either in the sandstone, or in the bituminated shiver connected with the blind coal.

Bituminous coal formed fromvegetables,

Bitumen, particles of coal, and remains of vegetables, I have always found in the sandstone strata that covered bituminated coal; and often, in the coal itself, vegetable remains occur. Hence I inferred, that such coal had been formed from vegetables; and the marks I formerly assigned of sandstone, including coal, applied only to bituminated coal. Blind coal appears to be *sui generis*, and to have been formed without the aid of vegetables.

blind coal not.

But without pretending to assign the mode of its formation, I think I am warranted to assert, it was not formed

formed in the way our fiery philosophers allege; and that the facts stated are fatal to their theory, as far as it depends upon pressure, or defect of pressure.

The strata of white sandstone, and of bituminated shiver, occupy the coast only a short way, on the north of the coal field; but on the south they prevail for several miles.

IX.

Questions respecting the Vines and Wines of Champagne, by Mr. Chaptal, *with Answers to them by* Mr Germon, *of Epernay**.

THE country that produces the celebrated wine known by the name of champagne is particularly famed for two kinds; the white, called wines of the river Marne; and red, or wines of the mountain of Rheims. If the southern aspects of the hills on the Marne produce excellent white wines, their backs and declivities, called the mountains of Rheims, though generally facing the north, and almost always the east, yield red wines of good and sound quality, and of a fine and high flavour, which ought to be made known.

Red and white champagne wines.

The side toward Rheims is divided in trade according to the quality of its wines into the mountain, lower mountain, and St. Thierry. Of the first those of Verzy, Verzenay,

Divisions into different qualities.

* The numerous facts here given render this Paper valuable, though the Author's theory and expressions are not always on a level with the present state of chemical knowledge. Nothing however more perfect, or more copious, has yet been published, respecting one of the three principal wine-countries in France. Mr. Chaptal will introduce almost the whole into his Art of Making Wine, which will appear in the course of the year.

[We have considerably abridged this Paper, from the Annales de Chimie, vol. LXI, p. 5, for January, 1807, taking only the principal facts; which we apprehend will not be unacceptable to many of our readers, as the practice of making wines for domestic use has much increased of late years; and many hints for the management of wines, and of the vine, may be derived from the information here given by a man of much practical knowledge.]

and

and Mailly, are most esteemed: the rest, though good, are not equal in quality. The vineyard of Bouzy, which terminates the chain, and the horizon between the south and east, so that it belongs to both divisions, must not be omitted. It produces excellent red wines, participating, from its situation, in the good qualities of those of Verzenay, and the good red wines of the Marne.

The lower mountain comprises a great number of vineyards, among which are distinguished those of Chamery, Ecueil, and Villedemange. The latter in particular produces wine in a favourable season, that will keep ten or twelve years. This division extends to the banks of the Aisne, but it produces only common wines.

The wines of St. Thierry are very pleasant, of a light colour, and much sought after in commerce. But the *clos* St. Thierry, from the archbishopric of Rheims, is the only one that unites the colour and flavour of burgundy with the lightness and briskness of champagne. It is to the champagne wines what the Clos-Vougeot is to burgundy.

What is the best aspect for vineyards?

The best aspect for vineyards is unquestionably the east and south. Of situations the midway of a hill is preferred, as the heat there is more concentrated, while it is exempt from the variations of the air on the summit, and the damp vapours of the foot. A western aspect is unfavourable to vegetation, which it burns and dries up: so that a vineyard with an eastern aspect is more valuable by one third.

What is the best soil?

Next to aspect in importance, if not before it, is the nature of the soil. This should be light, sandy, and granitic; neither compact, close, nor clayey. In general the vineyards of Champagne have a substratum of chalk: a kind of soil on which the vine grows slowly, but when it is once thoroughly rooted on it, it thrives well.

When and how are the vines planted?

The vines are planted in November or December, when the weather permits. An oblong hole or trench is made a foot and half deep, and two or three feet long. Into this the plant is introduced, and covered with earth, inclining it so, that only two or three inches of the extremity rises above the surface; and this extremity is *refreshed* by cutting it lightly in a horizontal direction. These trenches are made in rows, a foot and half from each other in strong

grounds,

grounds, and two feet in light. A distance of three feet is left between the rows, and the plants in one are placed opposite the intervals of the other.

Propagation by layers.

They are propagated by layers: for which purpose a turf should be cut from a meadow, or a marsh; the branch to be laid should be introduced into a hole made in the middle of this turf, and then fixed in the ground with it, in a sloping position. The root will form in the course of the year, and then the layer must be cut off close to the stock, and taken up with its turf.

Grafting disused.

Grafting is almost out of use. The fruit indeed is larger, but it is much more liable to fail from the slightest mismanagement, does not produce so sound a wine, and the vines do not last so long by far.

How long does a vineyard last?

A good vineyard will continue to produce well for fifty or sixty years, and frequently more, if well managed. If the layers be not planted deep enough, the vineyard will be covered with trailing roots, forming a sort of floor, so that there will be no place to lay down fresh shoots, and it must be broken up.

What grapes are most proper for white wine?

For white wine black and white grapes are planted indiscriminately in the same vineyard, which is perhaps wrong, as they do not ripen at the same time. But wine made entirely from black grapes would be too strong, and apt to become pricked in hot years: and entirely from white it would be too mellow, as these grapes contain more mucilage than the black.

Black generally preferred.

The kinds of grape cultivated are not many. The black are generally preferred for several reasons. They are not so soon spoiled by frost or rain, which are common about vintage-time; and they give more strength and body to the wine. Yet there are some places, where the wine is much esteemed, though they have few black grapes.

Danger from spring frosts.

As there is no danger from the frost in spring but at sunrise, an eastern exposure has most to apprehend from it, but no aspect is exempt from the danger. No means of guarding against it have yet been discovered.

Management of the vineyard.

On resuming the labours of the vineyard, about the end of February or in March, the first thing to be done, and one of the most essential, since on it depends the greatness

of

Pruning. of the crop, is to prune the vine. When it is strong, two side shoots or branches may be left: if weak, only one. Three eyes should be left on each shoot. Sometimes the vinedressers leave three shoots, and four eyes to a shoot. If the vine be young, and the stock is not loaded with old cuttings, its height when pruned is only three or four inches. The branches are never suffered to shoot up above a foot and half.

Digging. After this pruning, about the end of March, or in April, when the earth is softened and rendered pliable by the winter frost, it is dug up about a foot deep, so far as to uncover the roots, and all the clods are well broken. After this layers are put down where necessary, throwing on them a basket of dung, and filling up the trench with mould; so as to let the two or three stems left on them appear above ground four or six inches apart, and taking care not to injure the buds on them. At the end of April, or in May, the earth is stirred again, but more superficially. While the vine is in flower, it must not be touched: but when this is over, in June, the shoots are to be stopped at about a foot and half; and the vine is to be staked, and tied up, but not so as to interrupt the circulation of the air, or the developement of the shoots. When this is done the earth has a second stirring, and about the middle of August, it has its third and last.

Putting down layers

Stirring the ground.

Stopping the shoots, and staking.

Stirring the ground again.

Ripeness of the grapes. About the end of September, or later in some seasons, the grapes will be ripe; which is known by the footstalk of each being brown and woody, as well as the general stalk; the grape coming off easily, and the part of the stalk within it not appearing green; and the stones being brown, dry, and not glutinous.

Gathering them for white wine. Great care is necessary for making the white wine. The ripest and soundest bunches must be carefully gathered, freed from all dry, rotten, and bruised grapes, put into large baskets, covered with a cloth to keep the sun from them, carried into the shade, and there kept till the evening, when they are to be pressed as speedily as possible.

Pressing. The grapes being laid on the bed of the press, they are to be covered with three or four layers of flat stones, and the press turned. When the juice has run for four or five minutes,

nutes, the press is to be turned backward, the stones removed, the grapes that have protruded thrust into the heap, the stones replaced, and the press turned again. The juice from three of such pressures, which will not take up an hour, is put by itself for prime wine into a vat, where it is left all night to settle.

Putting into casks.

The next morning this juice is poured off from the sediment, and put into new, matched, and well rinced casks. In these it ferments, at first violently, afterward imperceptibly; till, about the end of December, having gone through all the stages of depuration, it becomes fine. It is then racked off, in dry weather, and on some fine frosty day, and fined with isinglass. About a pound of that of Marseilles is sufficient for 40 puncheons, of 200 bottles each. The isinglass being dissolved is well beaten, diluted with wine taken from the cask, then poured into it, and the whole well stirred by an instrument introduced at the bung-hole. The wine thus left to settle ferments slightly again, till it is stopped by the cold weather, or by time. In a month or six weeks it is racked off again, and has another fining with half the quantity of isinglass.

Racking and fining.

Second racking and fining.

Bottling.

In this state it usually remains till March, when it is bottled. Good glass bottles are taken for this purpose, well rinced, corked with superfine corks, and these are confined by packthread or wire. The bottles are then piled on their sides, one upon another, in the cellar.

As the fermentation is not completely terminated at the time of bottling, it revives about the middle of Angust, between which time and the end of September it is not unusual to have five or ten bottles in a hundred burst; and this continues till the March following, when it becomes more violent, or more moderate, according to the state and quality of the wine. In general, when not more than twenty bottles in a hundred burst during the whole course of the fermentation, the proprietor does not complain.

Decanted into fresh bottles.

Fifteen or eighteen months after the first bottling, when the wine has gone through all the stages of fermentation, and is to be sold or sent abroad, it undergoes a fresh decantation, which requires some dexterity. If the wine be not mantling [*mousseux*], it is simple enough. The bottle

is

is taken up in the position in which it lies; the wire is removed with a hook, which the man holds in his hand; the cork is drawn; and a well rinced, empty bottle being held perpendicularly to its mouth, the wine is decanted, leaving at the bottom the sediment, which had not been shaken. Some employ a siphon to draw off the wine in this case.

When the wine is mantling, the operation is much more nice and tedious. Boards are prepared, with holes at certain distances to receive the bottles, and placed near the pile. A workman carefully takes a bottle from the pile, in the position in which it lay; shakes it with a gentle, slow, and regular motion, so as to get all the sediment down to the side of the bottle, and thence to the neck, without mixing with the rest of the wine; and then places it on the board in a sloping position. This is done regularly till the board is filled. Twenty-four hours after the bottles are moved again in a less inclined position, so as to bring the sediment down upon the cork. If this be done completely, without rendering the wine at all thick, it is placed in a perpendicular position, and the same is done with all the other bottles. He then takes them one by one, bottom upwards, stays them with his left arm, removes the wire with his hook, and carefully draws the cork. The fixed air expands; the wine forces out the sediment into a receiver; when instantly the workman turns up the bottle, which has let out only what was necessary to render the remainder perfectly clear, and gives it to another, who fills it, and recorks it.

Will keep from ten to thirty years.

This wine, when sent abroad, will keep ten years, without its quality being impaired: but in cellars, particularly those of Champagne, w hc are cut out of the chalk rock, it will keep twenty or thirty years. The temperature of the cellars should be equable, and currents of air in them avoided.

Gathering the grapes for red wine.

For making red wine, the grapes are gathered with the same precautions as for making white, taking only the black grapes. These are bruised in particular vessels, by

Treading them.

men treading on them with strong wooden shoes: part of the stalks are thrown away, and the must is left in covered

Fermenting.

vessels to ferment sufficiently to extract the colouring matter from

from the pellicles. In some years three or four days are sufficient for this; in others it requires ten, fifteen, or even twenty.

When the fermentation begins, the husks and stalks are forced down so as to be entirely covered with the must, either by means of stout poles furnished with cross pegs; or, which is better, by a couple of stout men going into the vat, and well treading and mixing its contents. When the air above the vat extinguishes a candle; the stalks and husks rise forcibly, whatever pains be taken to sink them frequently, that the must may not acquire a disagreeable taste; the must experiences a degree of real ebullition; and the colouring matter is sufficiently decomposed: the fermentation must not be carried farther, lest the wine acquire a dry and hard taste, not to be cured even by keeping. Management of the fermentation.

The liquor is then to be drawn off into another vat; and the marc pressed, but only twice or three times. What runs from the marc being well mixed with the other, the whole is to be tunned into new, well hooped casks, previously rinced with hot water; but these must not be quite filled, as the wine still ferments for some days. As the fermentation abates, they are filled up, and the bung, in which a little hole is made, is put in. When the fermentation is become imperceptible, the cask is stopped close; care is taken to fill it up from time to time, for there is soon a vacuity formed in it, and even to open the bung. Pressing. Putting into casks.

About the end of December, in dry weather, and if possible on a fine frosty day, because all fermentation has ceased, the wine is racked off from the lees. About the middle of May, before the hot weather comes on, it is racked off again, and the barrels are fresh hooped, and the wine is put into the cellar. Racking. Second racking.

When the wine is to be sent off to the consumer, or put into bottles, it is fined. For this purpose the wine is racked off a third time; and the whites of five or six very fresh eggs are well beaten up in a pint of water, without making them froth if possible, for every puncheon holding 240 bottles. Those for white wine hold only 200. This is put into the cask, and stirred about, as in fining white wine. Third racking and fining.

The wine is generally bottled in November, or thirteen months Bottling.

months after it was made. Some excellent and generous wines may stand on the lees three or four years; but they should be kept in barrels that will hold eight or ten hogsheads, when the wine will feed, and be the better for it.

Keeps from six to twelve years.

Good red champagne will keep in bottle six, eight, ten, or twelve years.

Management of tall vines.

In some places, the vines are suffered to grow much higher than in others, or to about five feet, but this is adapted only to a strong and vigorous soil. For this purpose the strongest shoot of the vine is taken, all the rest being cut off, and all the lateral shoots. This is bent round in a complete circle quite to the stock, at the time when the sap is most abundant, and the buds already opened; and supported by an oaken prop six feet high, and an inch square, to which it is fastened in two or three places.

Laying down.

Vines of this kind are propagated by laying down the old stocks every ten or fifteen years, in a small long trench; leaving on them three or four branches, which are likewise buried in the ground. These will produce good plants for the following year.

Produce greater but not so good.

The produce of these vines is greater, but the grapes do not ripen so early, and the wine in consequence is not quite so fine and exquisite.

X.

On the most sensible Reagents for Muriatic, Carbonic, and Sulphuric Acids, and for Ammonia: by C. H. Pfaff, *Professor of Chemistry at Kiel**.

Sensible test of muriatic acid desirable.

In the inquiry concerning the pretended formation of muriatic acid in water, by means of the galvanic pile, it is unquestionably of great importance, to possess a very sensible test of this acid, that we may discover the first traces of it, and pursue its successive increase. Hitherto the nitrate of silver has generally been employed. This reagent is no doubt very sensible to this acid. Kirwan asserts, that one part of the acid diluted with 108333 of water may be de-

Nitrate of silver.

* Annales de Chimie, vol. LXII, p. 19, April, 1807.

tected

tected by its means; but this test is greatly surpassed by the solution of *mild nitrate of mercury* prepared cold. One part of muriatic acid of the specific gravity of 1·15, diluted with 70000 parts of water, barely exhibits a slight opaline hue, when tested with nitrate of silver. Diluted with 80000 times its weight of water, it eludes the action of this test, as well as of all others, except the mild nitrate of mercury, which renders it very perceptibly turbid. Its sensibility is so great indeed, that even $\frac{1}{300000}$ of a grain of muriatic acid at 1·15 is indicated by a slightly dull tint in the water that contains this extremely small quantity. From reflecting on the absolute insolubility, as it may be called, of the mild muriate of mercury, I was led to experiments concerning this reagent.

Mild nitrate of mercury much superior.

Indicates $\frac{1}{300000}$

It is at the same time the most sensible test of ammonia. One part of this alkali, diluted with 30000 of water, is indicated by a slight blackish yellow tint, when a solution of nitrate of mercury at a minimum of oxidation is added to it.

Detects $\frac{1}{30000}$ of ammonia.

Lime water, or barytes water, is generally considered as the most sensible test of carbonic acid. I have found, that the acetate of lead surpasses both. I was led accidentally to make this observation. Some distilled water, which I kept in a cellar not very deep under ground, where however there were no fermented liquors, was rendered very sensibly turbid, by adding this solution. Kirwan has accused the acetite of lead of being a deceitful test, his solution, which had been kept a little time, being sometimes rendered turbid by pure distilled water. But it is not deceitful; the water in this case is not pure, it contains a little carbonic acid. I prepared some distilled water free from all carbonic acid. It was not rendered turbid either by limewater, or by the solution of acetite of lead. I passed into it a few bubbles of carbonic acid, which acidulated the water so slightly, that it neither reddened litmus paper, nor rendered limewater turbid; but the solution of acetite of lead whitened it perceptibly.

Acetate of lead most sensible test of carbonic acid.

Not deceitful.

Acetate of lead is much less sensible to other acids. A solution of sulphuric acid at 1·85, diluted with 16000 parts of water, which acts sensibly on litmus paper, is not rendered turbid by acetate of lead. Barytes water however

Not so sensible to other acids.

detects $\frac{1}{70000}$ of its weight in water acidulated with sulphuric acid, and surpasses in sensibility for this acid all other reagents.

Mild nitrate of mercury a test of phosphoric acid.

The mild nitrate of mercury is almost as sensible a test of the phosphoric as of the muriatic acid; with this difference, that the precipitate with the former is soluble in an excess of phosphoric or nitric acid, but that with the latter is absolutely insoluble in an excess of any acid whatever.

XI.

Some farther Remarks on the pretended Formation of Muriatic Acid in Water by the Influence of the Galvanic Pile: by Professor PFAFF, *of Kiel.*

Inquiry into the formation of muriatic acid by galvanism. Apparatus.

I HAVE continued my researches into the pretended formation of muriatic acid in water, by the influence of the positive pole of Volta's pile. I have employed glass tubes of various diameters, from one line to an inch. The tubes were closed at bottom, into which the conducting wires were cemented with sealing wax. The communication between the two tubes, into one of which the influence of the positive pile was conducted, while that of the negative communicated with the other, was made at the top, sometimes by wet paper, sometimes by linen threads, sometimes by tendons, and sometimes by muscular fibre. I likewise varied the metal of the wires, employing successively platina, gold, silver, copper, and iron.

No traces of muriatic acid.

In all my experiments I could never obtain the least trace of muriatic acid, though my test, the mild nitrate of mercury, the most sensible of all for this acid, would have indicated the presence of $\frac{1}{300000}$ of a grain. But I found by litmus paper indications of an acid; which certainly was neither the muriatic, sulphuric, carbonic, nor phosphoric; since the nicest tests of these acids, which greatly exceed litmus in sensibility, gave no signs of their presence. In all probability therefore, it could be nothing but the nitric acid.

Some acid however.

Probably nitric.

Ammonia too. Both from the azote in the water.

I always obtained traces of an alkali too, which from every test was ammonia. I cannot therefore but adhere to my opinion, that the acid and alkali are formed at the expense of the nitrogen adhering to the water; which on one side unites with oxigen, on the other with hidrogen.

XII.

XII.

Description of the Mode of making Threshing-Floors in the Commune of Valbonnais, in the Department of the Isère: by Mr. J. J. Champollion Figeac, *Secretary to the Society of Sciences and Arts at Grenoble, &c.* *.

THE gypsum quarries of Valbonnais furnish two sorts of plaster, one white, the other red. The white is found only in solitary strata, not very abundant: the red, which is coloured by oxide of iron, is the most plentiful, and used almost exclusively for threshing floors.

Two sorts of gypsum. The red used for floors.

For this purpose it is burned for 24 or 30 hours, pounded as fine as possible, and in this state left for ten days, before it is used. It is to be observed, the more it is burned, and the finer it is pounded, the better it is. At the expiration of this term, and after the ground on which the floor is to be formed has been made very level, the plaster is to be diluted with cold water in a bucket. It must be carefully mixed so as not to have any lumps.

Calcined, powdered, exposed to the air, well mixed with cold water.

Two feet from one of the walls of the barn, and parallel to it, a ruler is to be placed, of the height which the plaster floor is intended to have. This is commonly two inches and half, or three inches. When the plaster is quite smooth, has acquired a certain degree of consistency, and is almost beginning to dry, it is poured out on the space between the wall and the rules. To level it another ruler is passed over it, one end of which rests on the former, the other touches the wall. It is then gone over with a trowel, to make it as smooth as possible, every vacuity is filled up, and any heterogeneous matters, that may be on the surface, are removed. Thus a smooth level surface is given to it, which is an essential quality.

A slip of wood placed two feet from the wall, and the plaster just hardening laid in the interval and smoothed.

As soon as this is done, a similar quantity of plaster prepared in the same manner is laid at the end of the former, and the same operations are repeated, till the plaster is extended to the opposite side of the barn. Here, however, it is absolutely necessary, to leave a little void space,

This is repeated till within a few inches of the opposite wall.

* Sonnini's Bibliothèque Physico-économique, Feb. 1807, p. 315.

to guard against the inconveniences that would ensue from the plaster swelling when in contact with both walls. This space may be three inches in a length of twenty feet. Other layers of plaster are then formed in succession by the side of this, bounding them always by the long ruler, placed at two feet distance from each preceding layer, which will keep them all of an equal thickness; and thus the whole of the floor is completed.

The board removed two feet farther, and another layer formed; and this repeated till the space is covered.

Great care must be taken, that the successive portions unite well together, that there may be no vacuity between them. For this purpose it is necessary to finish the whole in one day if possible: and to accomplish this a sufficient number of men should be employed in diluting and preparing the plaster, that those who are forming the floor may proceed without interruption.

Every portion must be well united, and the whole finished in a day if possible.

Ten days afterward the vacuity left between the floor and the wall is filled up, and then it will be ready for use. If in this time it acquire a deep red colour, it is a good sign. Such a floor will last in common a hundred and fifty years; and still longer, if it be not exposed to damp. When its surface becomes injured by time, and is no longer as smooth as it ought to be, all the plaster may be removed, exposed to the weather for a fortnight, burned again as if it were fresh taken from the quarry, pounded, mixed with water, and relaid in the same place, proceeding exactly in the same manner as when it was laid down the first time. The floor thus remade will last as long as it did before.

Ten days after the vacuity filled up.

Will last 150 years,

and then may be taken up, burned afresh, and laid down again.

Will then last as long as before.

The advantages of such a floor may readily be conceived, when the high price to which timber has risen of late years is considered. That some calculation of its cost may be formed, a square fathom of this floor, three inches thick, will require about eleven hundred weight of gypsum; and two men can work up seven times this quantity in a day.

Quantity of materials and labour.

SCIENTIFIC NEWS.

French National Institute *.

MR. LAPLACE has investigated the phenomena of capillary attraction; but instead of copying what Mr. Delambre says on this subject, we shall refer our readers to p. 164, 169, and 286 of the present volume for what has been done by this celebrated mathematician; and to our next number for some remarks on it by a learned correspondent. Laplace on capillary attraction.

In 1784 Mr. Roswag of Strasbourg presented to the board of trade some gauze made of iron wire, for which he received a reward; and the loom he invented for making it was lodged in the collection of machines of Vaucanson. In 1799 Mr. Rochon made others, and coated them with a transparent glue, to be substituted instead of horn for ship lanterns to be used between decks, and in engagements by night. He has since conceived, that with a thin coating of plaster they might be employed to preserve ships from fire, and buildings on shore still more easily; or at least that they would render the ravages of fire less frequent, and less terrible. These gauzes might be very useful too for theatrical decorations, which would not be liable to take fire. Their only inconvenience is their being so little flexible; but Mr. Rochon does not despair of means being found by chemistry to remedy this imperfection, and it was with a view of calling attention to this subject, that he read a paper on it to the class. Wire gauze coated with glue as a substitute for horn in ship lanterns. Security against fire. Safe stage decorations.

An eclipse of the sun is among the most useful phenomena for the verification of astronomical tables, or for the determination of the longitudes of places. It is likewise one of those, that most attract the attention of observers. Mr. Lalande, true to the custom he has followed these fifty years, has calculated all the observations he could collect of the eclipse of 1806. Clouds concealed it from the astrono- Total eclipse of the sun.

* Abridged from the account of the proceedings of the mathematical division of the class of mathematical and physical sciences given by the perpetual secretary, Mr. Delambre.

Observed in America.

mers of Paris; but it was seen in several parts of France, Germany, Holland, and Italy. In America it would be particularly interesting, as at Boston and Albany it was total. At Kinderhook, near Albany, it was observed by Mr. Ferrer with excellent instruments. He concluded the conjunction to be at 45 min. 33 sec. after 11. Mr. Lalande found precisely the same: and as he learned by other observations, that it happened at 30 min. 6 sec. after 4 at Paris, it follows, that the difference of longitude of these two places must be 7h. 15′ 27″ of time.

The eclipse was observed at Albany too, but at the instant of the return of the light the observer had not his eye at the glass: and though this phenomenon would appear to be of a nature to be seen as accurately with the naked eye, it seems to have been noticed a few seconds too late.

Disk of the moon illumined from its atmosphere.

A curious remark of Mr. Ferrer is, that the disk of the moon appeared illumined a few seconds before the end of the total eclipse, which seemed to him an effect of the atmosphere of the moon.

Only six stars visible, the sun being surrounded by a luminous ring.

The darkness was not so great as was expected. Only six of the principal stars or planets were seen. A luminous ring of 45 or 50′, surrounding the sun, diminished the obscurity.

Irradiation of the sun 2″. The diameter of the moon 2″ more than Lalande had calculated.

From the comparison of this total eclipse with some annular eclipses observed before, Mr. Lalande thinks, that the irradiation of the sun is 2″; and that 1″ must be added to the semidiameter which he had assigned to the moon from direct observations made at the full.

The sun movable in space,

Many astronomers have supposed, that the sun is not immovable in space. Mr. Lalande conjectured from its rotatory motion, which is unquestionable, that it has a movement of revolution. What he suspected Herschel has endeavoured to prove by observations. Mr. Prévot, of the academy of Petersburg, has been led to the same result: but Mr. du Séjour, having treated the question analytically, has found, that it is insolvable when considered in its totality. The results to which Mr. Herschel has been led by the apparent motions of different stars * do not accord suffi-

* See our Journal, vol. XIII, p. 59, and XV, p. 232, and 269.

ciently

ciently to establish the motion of the sun, and immobility of the stars: it rather appears, that they are all in motion: and it is on this supposition, that Mr. du Séjour declares the problem insolvable.

and the stars likewise: so that its motion cannot be determined.

Notwithstanding this decision, Mr. Buckhardt has subjected it to analysis anew. His formulæ are more commodious, and more easy of application, than that of Mr. du Séjour; and are less laborious than the trigonometrical calculation of Mr. Herschel. He has very adroitly eliminated the distances of the stars, which appear to be, and really are, one of the elements of the calculation, and which will probably remain for ever unknown to us. If the sun alone be in motion, this motion may be known to a certain degree of accuracy, in time, by means of good observations: but if the stars too move, the separation of the unknown quantities will be impossible, and some embarrassment will ensue to future astronomers, should there be an interruption to observations for a few centuries; and should they attempt to calculate the celestial movements anew by comparing their observations with ours, after a period of ignorance of some duration. But even on this supposition, which is fortunately very improbable, it would only follow, that the observations of the 18th century would appear a little less accurate; which would not prevent them from furnishing much better helps, than we found in the small number of rude observations transmitted to us by the Greeks.

Burckhardt has examined the question anew.

The problem of finding the train of wheels necessary to represent the motions of the planets was resolved by Huyghens in a very complete manner by continual fractions, which have the advantage of furnishing approximate values, expressed by the smallest numbers possible, in every degree of approximation with which the artist may think proper to content himself. But to this every artist who attempts to construct a planetarium is not equal. Mr. Burckhardt therefore has pointed out to them calculations more easy, and sufficiently exact.

Problem of finding the train of wheels for a planetarium.

To these labours of the class may be added the reports made by its committees on the most curious and important inventions submitted to its judgment. On both these accounts we shall particularly mention the report of Carnot

Pyreolophorus.

on

on the machine invented by Messrs. Nieps, and called by them a *pyrcolophorus*. By this word, compounded from πυρ, fire, ἀιολος, wind, and φερω, to carry, the inventors intended to point out the moving powers of the machine, which are wind from a pair of bellows, and air suddenly expanded by fire*. Their object was to discover a physical power equal to that of the steam engine without consuming so much fuel.

A power equal to the steam engine with less consumption of fuel.

To form an idea of the manner in which they produce and call into action the sudden expansion of air, suppose a copper receiver to be firmly fixed to a horizontal table. To one of its sides is fitted a tube, by means of which a body of air is conveyed into the receiver. This air meets in its way a few grains of combustible matter, which it projects on a flame, where it enters into ignition. The inflamed matter, entering into the receiver, expands its contained air with great force, which is exerted against the sides, and pushes forward a piston, sliding in a second tube, fitted to one of the sides. This piston drives before it a column of water, or any other body exposed to its action; after which the piston returns of itself to its former place, and the machine, recovering its former state, is again ready to act as before. All these effects take place in five seconds of time.

Its mode of action.

In a trial made by the inventors, a barge loaded with nine hundred weight, and its bow presenting a resistance of six square feet to the water, ascended the Saône with a velocity double that of the stream. In another trial made by the committee, the pressure exerted on a piston of 3 inches square was equal to a weight of 57000 grammes (126lbs); the interior capacity was 21 cubic inches; and the consumption of fuel was only 0·32 of a gramme (5 grains).

Experiments with it.

The inventors mean to carry their first attempts nearer to perfection: but even in the present state of the machine, its violent concussions, the shocks it gives to what supports it, and the celerity of its motions, leave no doubt of the in-

* This principle has already been employed in our own country, and we understand its powers were found to be very great; but some obstacles occurred, that prevented it from being followed up. From the account given by Messrs. Nieps, however, it was not precisely in the same way as their contrivance, but on a simple and more scientific principle. W. N.

tensity

tensity and impetuosity of this new moving principle; and valuable results may be expected from it, when by repeated trials all the energy of which it is susceptible is imparted to it. Such is the opinion of the committee, and the class determined, that the whole of their report should be inserted in the historical part of its memoirs, to preserve the remembrance and date of the first trial of an invention that may prove highly important.

New scapements.

Mr. Pictet presented ten models of scapements from Messrs. Malley of Geneva, three of which belonged in part to Mr. Tavan, the artist who made them all. They displayed an inventive genius, and great merit in the execution.

Frame for weaving ribbed stockings.

Mr. Desmarets read an interesting report on a new machine for weaving ribbed stockings, invented by Mr. Bellemère. This is not above half as expensive as the English stocking-frame, and its movements are much lighter. Its advantages are confirmed by two years experience.

Variation and dip of the needle, and magnetic power.

From the learned researches of Mr. Coulomb, and the formulæ of de Borda and Laplace, we are now able to determine with sufficient precision, and without too many difficulties, the variation and dip of the needle, and the intensity of the magnetic forces. But these nice observations require perfect instruments, time, and an exact knowledge of the meridian of the place. The observations which navigators, to whom most of these are often wanting, have been able to make, are not to be depended upon sufficiently for us to infer from them with certainty the situation of the magnetic poles and equator, and the points where the magnetic equator intersects that of the earth. Mr. Biot however has attempted to determine, from the observations of la Peyrouse and von Humboldt, all these elements of the magnetic theory of the globe; and he has given the necessary formulæ for calculating what the variation and dip of the needle should be in any given place.

Mr Biot has attempted to ascertain the elements of the magnetism of the globe.

Humboldt and Gay Lussac have made many observations on the dip of the needle, and the magnetic power of the earth.

The journey which Messrs. von Humboldt and Gay-Lussac have since made in Italy, France, and Germany, has afforded them repeated opportunities of comparing their observations with the hypothesis of Mr. Biot. The difficulty of ascertaining the meridian prevented them from observing the variation of the needle at their different stations; but

but they observed the dip, and the number of oscillations made in a given time by a horizontal needle, whence by a very simple formula they deduced the number of oscillations it would have made in its true direction, and the intensity of the magnetic forces.

These did not coincide with Biot's hypothesis.

To exhibit the whole of their labour at one view, and the consequences deducible from it, Mr. Gay-Lussac has given a general table of the observations themselves, the geographical latitude and longitude of the place, the latitudes and longitudes referred to the magnetic equator according to the hypothesis of Mr. Biot, the dips calculated according to this hypothesis, and the differences they found between their observations and this calculation. To this he has added observations on the nature of the soil, and its elevation above the level of the sea.

The dips were all in excess.

It is to be remarked, that all these differences are in the same direction, the dips by calculation being from 3° 42′ to 5° 9′ too great. Admitting, that some of these differences may be ascribed to local circumstances, or the unavoidable errours of observation, it appears at least highly probable, that a more considerable part arises from the situation attributed to the nodes of the magnetic equator, and to the angle it makes with that of the earth. It will not be difficult to determine the corrections, that Mr. Biot's hypothesis requires, to agree much better with these new observations, and reconcile them with those from which he determined his first elements. It is to be presumed, that Mr. Biot himself will consider this as an object of sufficient importance to engage his attention, when he has finished the important and difficult undertaking, on which he is now employed *. To give this theory all the precision of which it is susceptible, it is much to be wished, that we had a series of observations made in remoter parts of the globe with the same care as those of von Humboldt and Gay-Lussac: but in the mean time we perceive, that the intensity of the

Meridian line extended to the Balearic islands.

* Messrs. Biot and Arago set off in September last to continue the meridian line to the Balearic islands, and finish the labours interrupted by the death of Mr. Mechain. In December they began the observation of the great triangle, which is to connect the island of Ivica with the coast of Valencia.

magnetic

magnetic forces increases with the latitude, as Mr. von Humboldt had already remarked on his American tour; for at Berlin it is 13703, while at Rome it is only 12642. It follows too from their labours, that the influence of the chain of the Alps was very feeble, if any thing. That of Vesuvius at the moment of the earthquake and eruption of 1805 was not much more perceptible, and this would appear to be owing rather to local circumstances, than to a particular magnetic centre.

The magnetic power increases with the latitude.

The influence of the Alps little or nothing.

That of Vesuvius during an eruption the same.

The description of the instruments employed in these observations, and the disquisitions entered into by Mr. Gay-Lussac respecting the best means of making them, cannot fail to add to the confidence, which the well-known accuracy and skill of the observers must naturally inspire.

From eudiometrical experiments, and the analysis of the air, Messrs. von Humboldt and Gay-Lussac had been led to suspect, that all gases might have the same capacity for caloric. This consequence, which appeared deducible from their observations, deserved a more scrupulous examination, which Mr. Gay-Lussac undertook on his return. His new experiments confirmed those before made, yet led him to an opposite conclusion. The gases he had observed with Mr. von Humboldt had in reality nearly equal capacities for heat, but it was wrong to ascribe the same property to all gases without distinction.

All gases supposed by Humboldt to have the same capacity for heat;

but this is true only with respect to some.

The apparatus contrived by Mr. Gay-Lussac was extremely simple. It consists of two equal globes, each with two tubulures, one fitted with a cock, the other with a very sensible spirit thermometer. The globes having been freed from moisture by dried muriate of lime, they were exhausted of air, and one was filled with the gas to be tried. The communication between the two balloons being then opened, part of the gas included in the first rushed into the second, till an equilibrium was established; and then Mr. Gay-Lussac carefully examined the changes of temperature indicated by the two thermometers.

Gay-Lussac's apparatus for determining this.

In the first experiment, the subject of which was atmospheric air, he saw with astonishment the thermometer rising perceptibly in the exhausted globe in proportion as the air rushed into it. This fact appears diametrically opposite to another

Air rushing into a vacuum gave out heat,

another well known, which is, that a volume of air included in the receiver of an air-pump continually absorbs caloric as it dilates under the rising piston. It may be said, that the vacuum in the second globe was not sufficiently perfect, and that the air remaining in it, being compressed by the additional quantity admitted, was obliged to give out part of the air it contained: but this explanation Mr. Gay-Lussac refutes, first by reasoning, and afterward by a direct experiment.

and this in proportion to its density.

If the alcohol ascend in the second thermometer, it descends nearly the same quantity in the first. Now if, after having exhausted the second globe, the communication between them be opened, the gas, equally distributed, will be reduced to half the density it had before; and one of the thermometers will be seen to rise, and the other fall, each in an equal degree, but less than before, in consequence of the diminution of density. And if, by repeating the exhaustion, the density be reduced to half what it was in the second trial, and consequently to $\frac{1}{8}$ what it was in its original state, we shall find the equal and opposite variations of the two thermometers still following the ratio of the density.

Other gases produced the same phenomena.

Similar experiments, made with particular attention, on hidrogen, oxigen, and carbonic acid gas, produced similar results; that is to say, the quantities of caloric absorbed in the first globe, and evolved in the second, were always equal to each other, and proportional to the density.

Contrivance for equalizing the time of transmission of the gas.

To render the experiments comparable with each other, it was necessary, that the time occupied by all the different gases in their transmission from one globe to the other should be the same. This Mr. Gay-Lussac effected by a contrivance equally simple and ingenious, which diminished the orifice of the connecting tube in the ratio of the square root of the densities: and thus the time of transmission for all the gases was found to be eleven seconds.

Of these experiments, which deserve the attention of the natural philosopher, and which Mr. Gay-Lussac purposes to verify and extend by farther observations, the following are the results, which however he offers with some diffidence.

General results of the experiments.

1. When a vacuum comes to be occupied by a gas, the caloric evolved is not owing to the little air that might be left in it.

2. If

2. If a communication be opened between two equal spaces, one a vacuum, the other filled with a gas, the variations of temperature, positive in one and negative in the other, are equal in quantity, but not in intensity.

3. In the same gas these variations are proportional to the change of density it undergoes.

4. The variations in different gases are so much greater, in proportion as their specific gravities are less.

5. The capacity of a gas for caloric in a given volume diminishes with the density.

6. The capacities of gases for caloric, in equal volumes, are so much greater, as their specific gravities are less. This consequence will be evident to those who know the experiments, by which Mr. Gay-Lussac had already proved, that all gases are equally affected by equal elevations of temperature.

Progress of thermometers compared, both mercurial and spirit,

Mr. Cotte, correspondent of the Institute, has compared the progress of several thermometers, both of mercury and alcohol, in various expositions, during the hottest days of the three memorable summers of 1802, 1803, and 1806.

in doors and out, in the sun and in the shade.

Two of these thermometers, one mercurial the other spirit, were placed out of doors in the shade, and facing the north; two others were exposed to the direct rays of the sun; and two were within doors. All of them were constructed with the greatest care, and under the inspection of different members of the Academy of Sciences. Before Mr. Cotte examined the effects of different exposures, he determined, by taking the mean of a great number of observations, the comparative motions of these thermometers in the same situation.

Differences between mercury and spirit greatest in the sun.

It follows from these experiments, that the differences between the mercurial and spirit thermometers are much more considerable, when they are exposed directly to the rays of the sun; which Mr. Cotte ascribes chiefly to the red colour of the spirit: and this difference is greater, the greater the heat.

Hourly variations.

The greatest hourly variation takes place from 6 to 7, and more especially from 7 to 8 in the morning; it continues diminishing till 11; thence it increases till 2; and between 2 and 3 it diminishes a little.

The

The difference between the mercurial and spirit thermometers exposed to the sun is nearly the same from 10 in the morning till 4 in the afternoon.

The greatest heat without doors not always on the same days as within.

The maximum of the thermometers within doors does not happen on the same days as that of the thermometers without.

Clouds affect spirit most.

A cloud passing rapidly over the sun suddenly sinks the spirit 2° or 3°, the mercury about 1° only. When the cloud has passed, the liquid rises as quickly.

Mercury most uniform.

The motion of the mercury is most uniform.

Time of greatest heat.

The maximum of the thermometers out of doors in the shade takes place from 2 to 3: that of the thermometers in the sun, from 3 to 4: and that of the thermometers within doors, from 6 to 7, in the afternoon.

Fluctuation observed.

When the heat is the greatest, a kind of fluctuation and agitation is observed in the mercury, and still more in the spirit, which causes them to rise and fall continually.

Relation between the distances of any five points in space, by Carnot.

Mr. Carnot has published a memoir on the Relation that exists between the distances of any five points taken in space, followed by an essay on the Theory of Transversals. This forms an interesting appendage to the Geometry of Position of the same author. In it will be found a number of useful or at least very curious theorems; analytical formulæ for resolving all the problems respecting a quadrangular pyramid, without supposing any knowledge but that of its edges. All these formulæ are symmetrical, and possess a degree of elegance, that will much please the geometrician. Some, it is true, may stagger the hardiest calculator, and much shorter solutions might be obtained by the skilful application of trigonometry; but each problem would require new considerations, which do not immediately present themselves to the mind, while here every thing flows in the clearest manner from a few known principles. This work therefore is a repository, whence the geometrician may derive expressions, that will facilitate the solution of very complicated problems. To give an idea of the calculations of the author, we shall quote the enunciation of one of the last problems, which is as it were the summary of those that precede: "Of ten right lines, joining any five points taken in space two by two, nine being given to find the tenth."

The

The Essay on Transversals is not less curious. The fundamental principle of this likewise may be found in the Geometry of Position; and it was one of the two, on which Ptolemy built all his spherical trigonometry. The word transversal is here employed to signify any right line, cutting the three sides of a right-lined triangle or their prolongations. An equation of remarkable simplicity expresses the ratio between the segments of the sides. Mr. Carnot immediately deduces from it three other formulæ of the same nature, which, transferred afterward to spherical trigonometry, are found to be the same as Ptolemy had deemed sufficient for the purposes of astronomy. He demonstrated them synthetically, according to the method of the ancients; and his demonstrations, enlarged by his commentator Theon, are not very complex. Mr. Carnot, after having demonstrated the first principle exactly in the same manner as Ptolemy, finds in our modern trigonometry more simple means for the others.

Carnot's Essay on Transversals.

After having coincided with the Greek mathematician, he extends the theory in various ways, applying it to plane and spherical quadrangular figures; to every polygon, plane or even oblique; and lastly to pyramids: applications that are perfectly new, and of which not the least trace is to be found either in Ptolemy, or in his commentator.

Mr. Lacroix has published a fifth edition of his Elements of Geometry.

5th edition of Elements of Geometry by Lacroix.

Mr. Haüy has published a second of his Elements of Natural Philosophy. The great and rapid success of the first edition renders it unnecessary for us to enter particularly into the plan and execution of a work, which its author has revised throughout, to enrich it with all the new discoveries, that have taken place in such a short interval. Thus we find in it Mr. Laplace's theory of capillary phenomena; Mr. Gay-Lussac's experiments on the dilatation of gases; and the researches of Mr. Biot into the relation between the refractive power of different substances and their chemical composition, which he has just finished.

2nd edition of Hauy's Elements of Natural Philosophy.

Lectures

Lectures at St. Thomas's and Guy's Hospitals.

Medical and surgical lectures.

THE autumnal course of lectures at these hospitals, will commence as follows:

St. Thomas's.

Anatomy and the operations of surgery, by Mr. Cline and Mr. Astley Cooper, Thursday, Oct. 1st, at 2 'clock.

Principles and practice of surgery, by Mr. Astley Cooper, Monday, October 5th, at 8 in the evening.

Guy's.

Practice of medicine, by Dr. Babington and Dr. Curry, Friday, October 2, at 10 o'clock.

Chemistry by Dr. Babington, Dr. Marcet, and Mr. Allen, Saturday, October 3, at 10 o'clock.

Midwifery and diseases peculiar to women and children, by Dr. Haighton, Monday, Oct. 5, at 8 in the morning.

Pathology, therapeutics, and materia medica, by Dr. Curry, and Dr. Cholmeley, Tuesday, October 6, at 8 in the evening.

Physiology, or laws of the animal Œconomy, by Dr. Haighton, Wednesday, October 7, at 7 in the evening.

Experimental Philosophy, by Mr. Allen, to begin in November.

Clinical Lectures on select medical cases, by Dr. Babington, Dr. Curry, and Dr. Marcet.

N. B. The several lectures are so arranged as not to interfere with each other in the hours of attendance; and the whole is calculated to form a complete course of medical and surgical instruction. Terms and other particulars to be learnt from Mr. Stocker, apothecary to Guy's Hospital, who is also empowered to enter gentlemen as pupils to such of the lectures as are delivered at Guy's.

Fourcroy's Philosophy of Chemistry.

A. F. FOURCROY, professor of chemistry at Paris, has published an enlarged edition of his "Philosophy of Chemistry," which is considered as the best elementary work on that science. A translation of it by Mr. W. Desmond, is in the press, and may be expected early in September.

INDEX.

INDEX.

A.

ACETIC æther, preparation of, 219
Acid, sulphuric, fabrication of, 41—gallic, facts towards a history of, 58—sulphurous, 303
Acoustics, experiments in, 211
Adhesion of bodies to the surface of fluids, 169
Ætheogamia of Palisot de Beauvois, 307
Æther, acetic, *see* Acetic æther.
Affinities of substances to fluids, 286
Alegar, premium for the improvement of, 232
Algæ, 309
Ambergris, on, 340
Anfrye, M. his analysis of ancient pewter, 311
Animal resistance to the effects of heat, 144, 216
Arran, Isle of, mineralogical and geological observations in, 344
Artillery, improved matches for, 31
Astringents, action of, upon solutions of iron, 58
Attraction and repulsion of small bodies floating on the surface of liquids, 164
Austin, Mr. J. account of his new weaving loom worked by steam and water, 175
Autographs from stone blocks 231
Auzilly, M. 10.

B.

Baduel, M. 227
Banks, Sir Joseph, his experiments on the influence of a high temperature of the atmosphere on the animal economy, 145
Baraillon, M. on ancient pewter, 311
Bartholdi's process for obtaining pure gallic acid, 59, 60, 62
Beauvais's system of the fructification of mosses and mushrooms, 307—His insects collected in Africa and America, 310
Berger, M. 215
Bergman on cast iron, 187
Berthollet, M. his researches into the nature and properties of the gallic acid, 58, 61.—On prussiate of potash, 89, 92, 109.—On capillary attraction, 174.—On cast iron, 187, 189.—On the degrees of oxigenation indicated by the colour of precipitates, 268.—On sulphurous acid, 303
Berthollet and Guyton on the fabrication of sulphuric acid, 45
Bigger, Mr. Walter, 178
Billardiere's, "Flora of New Holland," 309
Binocular telescope, 201
Blagden, Sir Charles, on atmospheres of high temperature, 145, 215
Blasting rocks, by means of sand, 227
Blende, on, 337
Boerhaave's theory of respiration, 143
Boiler, a newly-constructed one, for saving fuel, 5.—Improvement in, 312
Bonpland, M. 309
Bookbinders' cutting-press, improved, 336
Borda, M. his proposed matches for artillery, 32
Bosc, M. 307
Bouche, M. his experiment of the application of the powers of electricity to discharge cannon, 232

Brisson, Mr. his "Treatise on Specific Gravity," 191
Bucholz, M on the conversion of chalk by fusion into a substance analogous to marble, 229—On the red iron-stone of Suhl, 230—On the volcanic calcedony, 230
Bugs, method of destroying, 38
Buffon, 212

C.

Caddel, W. Esq. 5
Cadet, C. L. on wooden matches for Artillery, to be used instead of rope-match or port-fires, 31—His analysis of coffee, 113
Calcedony, volcanic, 230
Camera Lucida, 1, 312
Candolle, M. 227—On parasitic funguses, 308
Capillary attraction, 170, 286, 365
Carradori, Dr. 275
Cartheuser, M on the gallic acid, 58
Cast iron, 185
Cement to keep out wet from walls or floors, and for joining stone or marble, 142
Chalk converted into a substance analogous to marble, 229
Champagne, vines and wines of, 353
Changeux, M. on the resistance of the human body to the effects of heat, 146
Chaptal, M. 183—Questions by, respecting the vines and wines of Champagne, 353
Chenevix, M. on coffee, 121, 127—On the fusion of metals, 188
Chevreuil, M. 46
Cheselden, 213
Clark, Mr. J. on the use of gall in painting in water-colours, 341
Clement, M. *see* Desormes and Clement.
Cobalt, facts towards a history of, 46
Cochineal, on, 340
Coffee, extract from a dissertation on, 113
Cold, effects of on the animal system, 142
Colours, experiments on, 18—Combinations of, and the results produced therefrom by refraction, 204.—Ditto by reflection, 206
Condillac, 212
Condor, the, described, 310
Copies of written paper obtained by pressure, 178
Copying machine, 178
Corbet, Mr. Archdeacon, 74
Cotton dyeing and printing, 184
Crawford, Dr. on the effects of heat on the animal constitution, 147
Cross, Mr. Hugh, 178
Curaudeau, on Prussiates, 109
Curvilinear saw, 334
Curwen, Mr. Account of his drill horse-hoe, or weed-harrow, 281
Cuthbertson, Mr. communication from, on the electricity of saline bodies, 12
Cutting-press for Bookbinders, improved, 336
Cuvier, M. his inquiries concerning animals known to the ancients, 310

D.

Dalton, Mr. on the absorption of gases by water, 275
Damp walls, method of curing, 141
Darso, M. on the oxidations of iron, 221, 267, 328
Decomposition of Light, 18
Delametherie, J C. 229
Delaroche, F. F. his experiments on the effects produced by a high temperature on the animal economy, 142, 215
Delaville, Dr. on the oxidation of lead, 207
Delineation, new instrument for, 1, 312
Descotils and Hassenfratz's examination of sparry iron-ores, 311
Desormes and Clement on the fabrication of sulphuric acid, 41
Deyeux, M. his sublimation of the gallic acid, 58, 60

INDEX.

Dobson, Dr. on atmospheres of high temperature, 146
Drill horse-hoe for turnips, or weed-harrow, 281, 284
Drummond, Mr. John, 178
Duhamel, *see* Tillet.
Duntze, Arnold, his experiments on animals, to ascertain the degree of heat they were capable of bearing, 144
Dupont, M. de Nemours, on instinct, 299
Durosier, M. on the preparation of acetic æther, 220
Dyes, violet purple, 182
Dykes, Mr. J. D. B. 283

E.

Eclipse of the sun, total, observed in America, 365
Eel, electrical, of Surinam, 310
Electrical experiments, 11
Electricity, application of its powers to discharge cannon, 232
Ellis, Mr. H. his relation of a remarkable fact respecting heat, 143
Emeritus on looming or horizontal refraction, 153
Erman, M. his memoir on two new classes of galvanic conductors, 233, 316
Estremadura, silver mines of, 129

F.

Faro Islands, charts and description of, lately published, 231
Fernandez on the solution of muriate of silver, 188
Field, Mr. G. account of his stove for heating rooms, or drying different articles, 263
Figeac, Mr. J. J. C. his description of the mode of making threshing-floors in the commune of Valbonnais, 363
Filtering stones, 190
Fir-trees, remarks on pruning, 157
Flint sculptured, a curious antique, 195
Flint of recent formation, 230
Fordyce, Dr. his experiments on animal heat, compared with that of the atmosphere, 145, 215
Fossils penetrable by water, 193
Fossil remains of lost animals, 310
Foster, Mr. Matthew, 283
Fourcroy on gallic acid, 69—On the degrees of oxigenation, 268
Franklin, Dr. on the effects of heat on animals, 144
Fuel, economy in, derived from a newly constructed boiler, 5, 312
Funguses, 308

G.

Gaff, Thos. Esq. 283
Gall, on, 340
Gallic acid, facts toward a history of, 58
Galvanism, 149, 233, 316, 362
Gases supposed to have an equal capacity for heat, 371
Gay-Lussac, on the absorption of gases by water, 275
Gehlen, M. on the preparation of acetic æther, 219—*See* 229
Geometry, Elements of, by Lacroix, 375
Gerhard, on fossils penetrable by water, 193
Germon, M. on the management of vines and wines in Champagne, 353
Gillet-Laumont, Mr.—*See* Laumont.
Gioanetti, M. on the action of astringents on solutions of iron, 58
Grignon's refined cast iron, 186
Growth of trees in the Botanic Garden at Calcutta, 110
Guadalcanal, platina found in the silver mines of, 128
Guyton de Morveau, M. his experiments on the adhesion of bodies in fluids, 172—On filtering stones, and the method of determining the specific gravity of substances with large pores, 190—His report on a sculptured head of flint, 195—*See* Berthollet and Guyton.
Gy, M. And. de, on the structure of

Mount Jura, from a considerable number of heights, taken by a barometer, and extended through France to the sea, 341

H.

Haldat, Dr. on double vision, 201
Haller, on respiration, 143
Haquet, M. on the formation of flints, 230
Hardie, Mr. his improved bookbinders' cutting-press, 306
Harrow for weeding, 281
Hassenfratz, *see* Descotils and Hassenfratz.
Haussman, J. M. on violet purple, and the different tints that may be derived from it, 182
Hauy, M. on the apparent attraction and repulsion of small bodies floating on the surface of liquids, 168
Headrick, Rev. James, his mineralogical and geological observations made in the Isle of Arran, 344
Hearing, experiments on, 211
Heat, effects of on animals, 142.—Extraordinary instance of the power of resistance to, 143, 215.—Produced by a vibratory motion, 311
Henderson, Mr. Thos. 178
Henry, Mr. on the two different methods of preparing acetic æther, 219.—On the absorption of gases by water, 275
Hibernation of animals, prize question relative to, 306
Hildesheim, remark on two ancient candlesticks in the cathedral there, 131
Horizontal refraction, 153
Humboldt, M. on the absorption of gases by water, 275—*See* 309
Hunter, Mr. John, on the influence of heat on the animal frame, 146

I.

Ichneumon wasp, 302
Inks for writing, various compositions of, 180
Insects in houses, &c. on the means of destroying, 38
Instinct of animals, 299
Institute, French National, prize questions by, 306
Iron, oxidations of, 221, 267, 328
Iron-stone of Suhl, 230

J.

Jessop, Mr. his method of blasting rocks, by means of sand (described vol. ix. p. 320, and vol. xii. p. 60) adopted in France, 227
Jura, Mount, remarks on the structure of, 341
Jurin, Dr. on double and single vision, 201

K.

Kirwan, Mr. on filtering stones, 190
Klaproth's analysis of the pumice stone of Lipari, 194—Of a new fossil from Virieglach, in Stiria, 230

L.

Lagrange, Bouillon, his facts towards a history of the gallic acid, 58, 118
Lake made from coffee, 113
Lalande, M. on the tempest of Feb. 18, 1807, 152
La Marck, *see* Marck.
Lambert, on the substances contained in atmospheric air, 275
Laplace, M. 25—On the apparent attraction and repulsion of small bodies floating on the surface of liquids, 164—On the adhesion of bodies to the surface of fluids, 169, 286—On capillary attraction, 365
Laumont, M. Gillet, his improvement in the art of blasting rocks, 229
Lead, oxidation of, 297
Lectures, medical and surgical in London, 376
Lelièvre, M. on the carbonate of manganese, found with iron spars, 311
Lespagnol, Mr. 32
Levels of France, 341
Lewis, M. on the action of astringents upon solutions of iron, 58
Light, decomposition of, 18—Analysis of, 29

INDEX.

Linnæus on the filtering stone, 190
Loevenoern, M. his description and chart of the Faro Islands, 231
Loom worked by steam and water, 175
Looming, or horizontal refraction, 153
Ludicke, M. on fossils penetrable by water, 193
Lunar observations, 366

M.

Macquer, M. on the gallic acid, 58
Macvicar, Mr. 178
Magnetism, 369
Marck, La, on the tempest of the 18th Feb. 1807, 88, 151
Mastodontes, a genus of fossil animals, 310
Matches of wood for artillery instead of those of rope, 31
Mercury, machine for triturating, 313
Meridian line extended to the Balearic Islands, 370
Mineralogy of Arran, 344
Mitchell, Mr. S. on the soda, magnesia, and lime, contained in sea-water, 72
Monnet, M. on the gallic acid, 58
Monteith, Mr. J. his spinning works at Pollockshaws, 175
Morveau's experiments on zinc, 338
Mosses, fructification of, 307
Muriatic acid formed in water by galvanism, 362
Mushrooms, fructification of, 307
Mutrie, Mr. David, 178

N.

Natural philosophy, Elements of, by Hauy, 375
Neil, Mr. 178
Nemours, M. Dupont de, on instinct, 113
Newton's Optics, quotations from, 19, 203
Nickel, facts towards a history of, 53
Nollet, Abbé, 24

O.

Optics, experiments in, 201
Oxidations of iron, 221, 267—Of lead, 297

P.

Palisot de Beauvais, M. 307
Parmentier, M. his process for obtaining acetic æther, 221
Paysse, M. his memoir on coffee, 121
Pelletier, M. on the preparation of acetic æther, 219
Perronet, M. 191
Persten, Mr. M. 178
Petit-Thouars, M. 309
Pewter, ancient, 311
Pfaff, Professor, on the most sensible re agents for muriatic, carbonic, and sulphuric acids, and for ammonia, 360—On the pretended formation of muriatic acid in water by the influence of the galvanic pile, 362
Phillips, R. Esq his account of the improvement of an extensive tract of land, 74
"Philosophy of Chemistry," by Fourcroy, 376
Phosphorescent substances, prize question relative to, 306
Pictet, Professor, his improvement in the art of blasting rocks, 228
Planche, M. on sulphurous acid, 303
Planetarium, 367
Plants, East-India, 112
Platel, *see* Thillaye-Platel.
Platina in the mines of Guadalcanal, 128
Port-fires superior to rope matches for the use of artillery, 31
Prieur, Col. C. A. on the decomposition of light into its most simple elements, 18
Prize Questions by the French National Institute, 306
Proust, M. his proposed wooden matches for the service of the artillery, 32—Facts towards a history of cobalt and nickel, 46—Discovery of the properties of gallic acid, 58—Facts towards a history of prussiates, 89, 249—On cast iron, 185—His theory of oxidation, 221, 267—On blende and some other articles, 337
Pruning fir-trees, 157

Prussiates, facts towards a history of, 89, 249
Pumice-stone, analysis of, 194
Pyreolophorus, 367

R.

Reagents for muriatic, carbonic, and sulphuric acids, and for ammonia, 360
Redowski, Mr. his projected scientific expedition, 232
Refraction, horizontal, 153
Reid, Mr. James, 178
Repulsion, *see* Attraction.
Respiration, 143
Reuter, Mr. his invention of printing from autographs of stone, 231
Richter's process for extracting and purifying gallic acid compared with the inventions of other chemists, 58, 60, 71
Ritter, M. on muriatic acid and soda formed by galvanism, 231
Rockets discharged by means of electricity, 232
Rocks, method of blasting with sand, adopted in France, 227
Roxburgh, Dr. his table of the growth of trees in the Botanic Garden at Calcutta, 110
R. T. on the art of making copies of written papers by pressure, 178
Rumford, Count, description of his new boiler, constructed with a view to the saving of fuel, 5—His experiment on the use of the heat of steam in place of that of an open fire, in the making of soap, 10.—His adherence to the old theory of heat, 311

S.

Saline bodies, their habitudes with regard to electricity, 11
Salmon, Mr. R. on pruning fir-trees 157
Saussure, on the heterogeneous particles contained in air, 275
Saw, a curvilinear, 334
Say, M. 194
Scapements, new, for time-keepers, 369
Scheele's discovery of the means of separating the gallic acid, 58, 71.—On prussiates, 106, 109, 249—On acetic æther, 219.—His mistake respecting oxigenation, 274
Schul, J. H. Von, 134
Scientific News, for May, 88—June, 151—July, 227—August, 306, 365
Scientific voyage, 232
Sea-water, analysis of, 72
Segner, M. on capillary attraction, 174
Seguin, M. his method of freeing alum from iron, 311
Skrimshire, Mr. W. jun. on the habitudes of saline bodies, with regard to electricity, 12
Soap-boiling, improvement in, 10
Soehné, Mr. 183
Solar motion, 366
Solander, Dr. on the influence of extreme heat on the animal constitution, 145
Sound, experiments on, 211
Sphex, interesting economy of the, 302
Steam, boiler for generating, 5—Experiment on the use of, in the manufacture of soap, 10
Stereometer, 194
Stove for heating and drying, 263
Sulphuric acid, theory of the fabrication of, 41
Sulphurous acid, remarkable properties of, 303
Swimming Society in Denmark, 231

T.

Talaker, Mr. William, 189
Tatum, Mr. John, on the increase of temperature produced by the galvanic action, 149
Telescope, Binocular, 201
Temperature, increase of by galvanism, 149

Tempest on the 18th Feb. 1807, observations on, 88, 151
Tenon's "Memoirs on the Dentition of the Horse," 310
Tests of certain acids and ammonia, 360
Thenard, M. his discovery of white oxide of iron, 268
Thermometers, experiments on, 373
Threshing floors of plaster, 363
Thillaye-Platel, Anthony, on the carbonization of turf, a process by which all possible advantage may be derived from products hitherto neglected in that operation, 131
Tillet and Duhamel, on the effects of heat on animals, 144
Tilloch, Mr. A. letter from, on the improved bookbinders' cutting-press, 336
Torpidity of certain animals during the winter season, prize questions relative to, 306
Transversals, theory of, 374
Trees, table of the growth of, 110
Tremery, Mr. 25
Trituration of mercury, machine for, 313
Trotter, John, Esq. his curvilinear saw described, 334
Turf, carbonization of, 131

V.

Variation and dip of the magnetic needle, 369
Vapour-baths of Russia, 143
Vauquelin, M. his account of the platina found in the silver-mines of Guadalcanal, in Estremadura, 128—His analysis of the iron-ores of France, 310
Vaux, M. Antony Alexis Cadet de, on coffee, its history, properties, and the mode of obtaining from it the most pleasant, wholesome, and economical verage, 113
Ventenat's "Garden of Malmaison," 309
Vines, culture of in France, 353
Violet purple dyes, 182
Vision, experiments on, 201
Von Schule, J. H. 184

W.

Wailly, M. 191
Waistell, Mr. C. account of his drill horse-hoe for turnips, 284
Wallerius on the filtering-stone, 190
Wasp, ichneumon, interesting economy of the, 302
Waste lands, improvement of, 74
Water of the sea; observations on the soda, magnesia, and lime, contained in it, 72
Waugh, Mr. John, 178
Weaving frame for ribbed stockings, 369
White oxide of iron, examination of, 268
Wilson, Mr. Charles, his method of curing damp walls, by the application of a newly-invented composition, 141
Wines of Champagne, management of, 353
Wire Gause, 365
Wollaston, Dr. W. H. his camera lucida described, 1, 313—His investigation respecting horizontal refraction, 153, 156
Writing ink, 180
W. W. letter from, on the means of destroying the insects which infest houses in large towns, 38
W. X.'s description of a machine for triturating quicksilver and combining it with other substances, 313

Y.

Young, Dr. on the adhesion of bodies to the surface of fluids, 169

Z.

Zinc, on, 337, *et seq.*

END OF THE SEVENTEENTH VOLUME.

Printed by W. Stratford, Crown-Court, Temple-Bar